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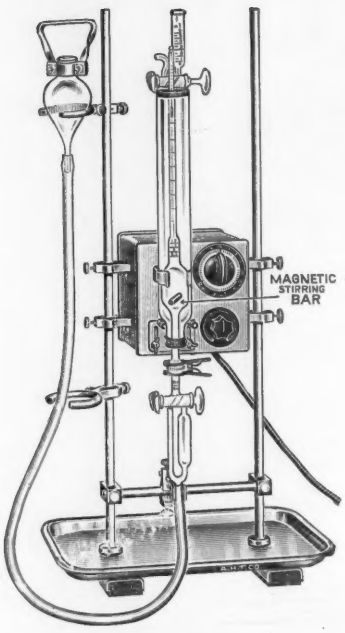


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## Once More with Feeling

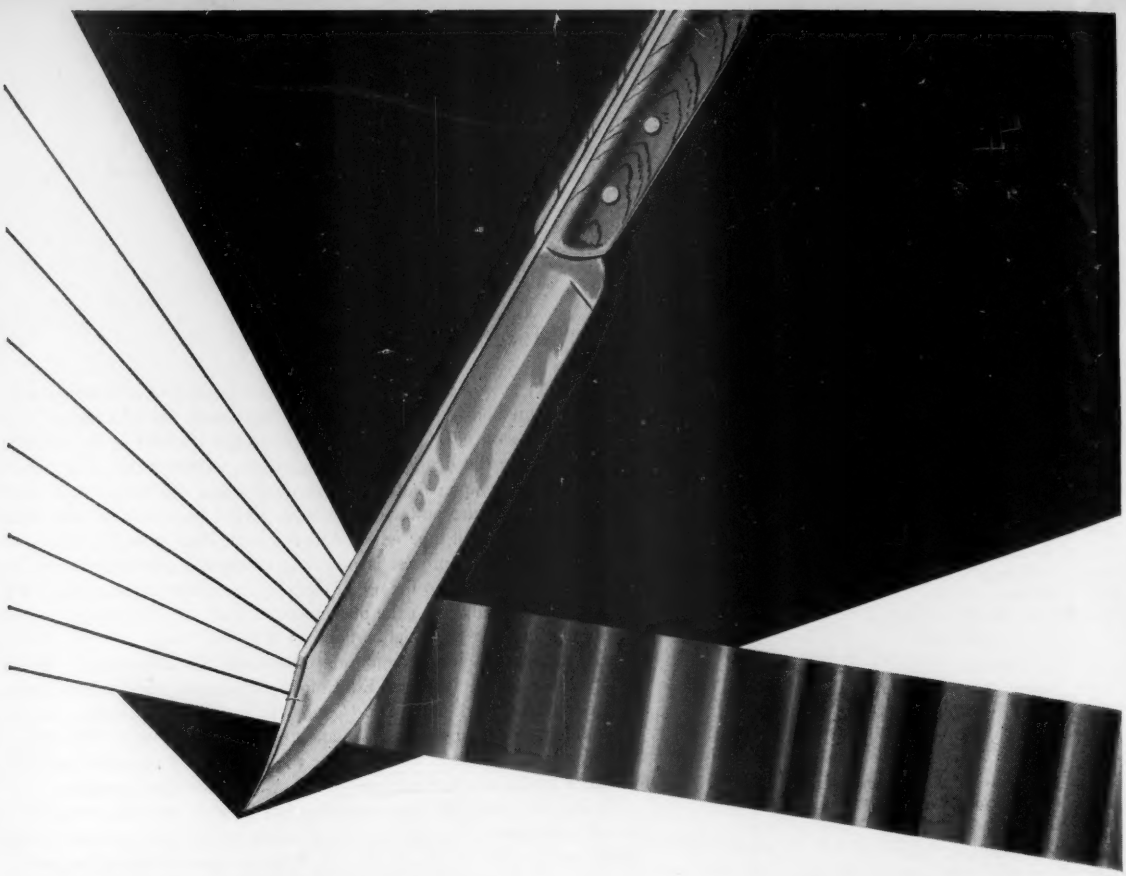
No matter how one may regard the general content of commercial television, this medium does have one important lesson for educational television. The lesson is that the use of a videotape recorder is the simplest way to improve the *quality* of a performance, whatever the subject matter. Immediately upon the completion of a scene, the 2-inch wide tape, carrying both picture and sound, can be played back on the instrument and, if necessary, the tape erased and the scene done over. No time-consuming development process is necessary before the performance can be viewed. The drawback to the use of videotape recorders in educational TV is that they are as hard on the pocketbook as they are easy to operate. An instrument costs in the neighborhood of $50,000.

Last month the Ford Foundation eliminated this drawback by giving $2.7 million to the National Educational Television and Radio Center, located in New York City. The Center will equip with recorders most of its 43 affiliated stations now on the air throughout the country. The Center will also supply stations with tape for the first year of operation and will purchase for its own use machines for copying outstanding programs.

In the two years the new invention has been in use commercially, it has demonstrated advantages over both filmed and live shows which should apply equally well to educational TV. Besides the advantage of improving performance by making possible immediate editing, tape has all the flexibility in programming of film together with the high fidelity of a live show. As with film, programs can be recorded at hours to suit the convenience of performers and studio workers and then presented on the air at hours to suit the convenience of viewers. And, like film, tapes can be reproduced rapidly or stored indefinitely. But where with film the process of development and projection means some loss in light values and detail, a taped show is practically indistinguishable from a live show.

Up till now, a lecturer in educational TV might spend hours studying notes and preparing demonstrations, and then make a single live appearance, with the show possibly being recorded for reruns on a low-fidelity film device. How much better the opportunity for developing the viewing side of television if the lecturer spends some of this time in actual presentation and editing. He could try a demonstration, watch it on the monitoring screen, and then do it again with improvements. In demonstrating centrifugal force, for example, he might choose to do again the bit where he twirls a pail of water about his head, but this time faster.

We have been more enthusiastic in the past about the use of private and government funds for the support of research than for the procurement of hardware, but we are glad on this occasion to reverse the familiar stand. To be sure, good research in educational TV is needed, for according to some observers, much of present research is not tapping any real variables, except that groups of students with TV teachers are compared with groups without such teachers. Good research is needed, but the time is also ripe for a substantial investment in hardware. By underwriting the cost of videotape recorders, the Ford Foundation has taken a step that should result in an immediate and general improvement in the effectiveness of educational TV.—J.T.



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CURRENT PROBLEMS IN RESEARCH

# Organic Compound Synthesis on the Primitive Earth

Several questions about the origin of life have been answered, but much remains to be studied.

Stanley L. Miller and Harold C. Urey

Since the demonstration by Pasteur that life does not arise spontaneously at the present time, the problem of the origin of life has been one of determining how the first forms of life arose, from which all the present species have evolved. This problem has received considerable attention in recent years, but there is disagreement on many points. We shall discuss the present status of the problem, mainly with respect to the early chemical history of, and the synthesis of organic compounds on, the primitive earth.

Many of our modern ideas on the origin of life stem from Oparin (*1*), who argued that the spontaneous generation of the first living organism might reasonably have taken place if large quantities of organic compounds had been present in the oceans of the primitive earth. Oparin further proposed that the atmosphere was reducing in character and that organic compounds might be synthesized under these conditions. This hypothesis implied that the first organisms were heterotrophic—that is, that they obtained their basic constituents from the environment instead of synthesizing them from carbon dioxide and water. Horowitz (*2*) discussed this point further and outlined how a simple heterotrophic organism could develop the ability to synthesize various cell constituents and thereby evolve into autotrophic organisms.

In spite of the argument by Oparin, numerous attempts were made to synthesize organic compounds under the oxidizing conditions now present on the earth (*3*). Various sources of energy acting on carbon dioxide and water failed to give reduced carbon compounds except when contaminating reducing agents were present. The one exception to this was the synthesis of formic acid and formaldehyde in very small yield ($10^{-7}$ $H_2CO$ molecules per ion pair) by the use of 40-million-electron-volt helium ions from a 60-inch cyclotron (*4*). While the simplest organic compounds were indeed synthesized, the yields were so small that this experiment can best be interpreted to mean that it would not have been possible to synthesize organic compounds nonbiologically as long as oxidizing conditions were present on the earth. This experiment is important in that it induced a reexamination of Oparin's hypothesis of the reducing atmosphere (*5*).

## The Primitive Atmosphere

Our discussion is based on the assumption that conditions on the primitive earth were favorable for the production of the organic compounds which make up life as we know it. There are many sets of conditions under which organic compounds could have been produced. All these conditions are more or less reducing. However, before accepting a set of conditions for the primitive earth, one must show that reactions known to take place will not rapidly change the atmosphere to another type. The proposed set of conditions must also be consistent with the known laws for the escape of hydrogen.

Cosmic dust clouds, from which the earth is believed to have been formed, contain a great excess of hydrogen. The planets Jupiter, Saturn, Uranus, and Neptune are known to have atmospheres of methane and ammonia. There has not been sufficient time for hydrogen to escape from these planets, because of their lower temperatures and higher gravitational fields. It is reasonable to expect that the earth and the other minor planets also started out with reducing atmospheres and that these atmospheres became oxidizing, due to the escape of hydrogen.

The meteorites are the closest approximation we have to the solid material from which the earth was formed. They are observed to be highly reduced—the iron mostly as metallic iron with some ferrous sulfide, the carbon as elemental carbon or iron carbide, and the phosphorus as phosphides.

The atmosphere under these reducing conditions would contain some hydrogen, methane, nitrogen, and ammonia; smaller amounts of carbon dioxide and carbon monoxide; and possibly small amounts of other substances such as higher hydrocarbons, hydrogen sulfide, and phosphine. These substances were probably not present in equilibrium concentrations, but compounds which are thermodynamically very unstable in this highly reducing atmosphere —such as oxygen, oxides of nitrogen, and oxides of sulfur—could not have been present in more than a few parts per million. This is true of compounds which are unstable in the present oxidizing atmosphere of the earth, such as hydrogen, ozone, methane, and nitrous oxide.

The over-all chemical change has been the oxidation of the reducing atmosphere to the present oxidizing atmosphere. This

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is caused by the loss of hydrogen, which results in the production of nitrogen, nitrate, sulfate, free oxygen, and ferric iron. As is discussed below, many complex organic compounds would have been formed during the course of this over-all change, thereby presenting a favorable environment for the formation of life. Whether the surface carbon of the present earth was all part of the initial atmosphere or whether it has been escaping from the earth's interior in a somewhat reduced condition is not important to the over-all picture.

## Escape of Hydrogen

We have learned in recent years that the temperature of the high atmosphere is 2000°K or more, and there is no reason to suppose that the same temperature was not present in the past. One might expect that a reducing atmosphere would be cooler than an oxidizing atmosphere because methane and ammonia can emit infrared radiation while the diatomic molecules, nitrogen and oxygen, cannot. Curtis and Goody (*6*) have shown that carbon dioxide is ineffective in emitting infrared radiation in the high atmosphere. This is due to the low efficiency of energy transfer from the translational and rotational to the vibrational degrees of freedom, and it seems likely that this would apply to methane as well.

The loss of hydrogen from the earth is now believed to be limited by the diffusion of $H_2$ to the high atmosphere, since almost all the water is frozen out before it reaches the high atmosphere. Urey (*7*) has discussed this problem and finds that the loss is entirely due to these effects and not to the Jeans escape formula.

The present rate of escape is $10^7$ atoms of hydrogen per square centimeter per second, and it is proportional to the concentration of molecular hydrogen in the atmosphere, which is now $10^{-6}$ atm at the earth's surface. This rate would result in escape of hydrogen equivalent to 20 g of water per square centimeter in the last $4.5 \times 10^9$ years. This rate is not sufficient to account for the oxygen in the atmosphere (230 g/cm²).

In addition, we must account for the oxidation of the carbon, ammonia, and ferrous iron to their present states of oxidation. The oxidation of the 3000 g of surface carbon per square centimeter present on the earth from the 0 to the +4 valence state (that is, from C or $H_2CO$ to $CO_2$) would require the loss of 1000 g of hydrogen per square centimeter. At the present rate of escape this would require $2.5 \times 10^{12}$ years. In order for this escape to be accomplished in $2.5 \times 10^9$ years (that is, between $4.5 \times 10^9$ and $2.0 \times 10^9$ years ago), a pressure of hydrogen at the surface of the earth of $0.7 \times 10^{-3}$ atm would have been required. In order for the nitrogen, sulfur, and iron also to be oxidized, even larger losses and a higher pressure of hydrogen would have been needed. We use a figure of $1.5 \times 10^{-3}$ atm for the hydrogen pressure in the primitive atmosphere.

These calculations are greatly oversimplified, since methane and other volatile hydrogen compounds would be decomposed in the high atmosphere and therefore a higher concentration of hydrogen might exist in the high atmosphere than is indicated by surface partial pressures. However, the results of the calculation would be qualitatively the same for hydrogen pressures different from the chosen value by an order of magnitude.

## Equilibria of Carbon Compounds

The partial pressure of $CO_2$ in the atmosphere is kept low by two buffer systems. The first system, which is rapid, is the absorption in the sea to form $HCO_3^-$ and $H_2CO_3$. The second, which is slow, is the reaction of carbon dioxide with silicates; for example

$$CaSiO_3 + CO_2 \rightarrow CaCO_3 + SiO_2 \qquad K_{25°} = 10^8$$

The partial pressure of $CO_2$ at sea level ($3.3 \times 10^{-4}$ atm) is somewhat higher than the equilibrium pressure ($10^{-8}$ atm), but very much lower than would be the case without the formation of limestones ($CaCO_3$).

The equilibrium constant at 25°C in the presence of liquid water for the reaction

$$CO_2 + 4H_2 \rightarrow CH_4 + 2H_2O$$

is $8 \times 10^{22}$. Assuming that equilibrium was attained, and using partial pressures $P_{CO_2} = 10^{-8}$ atm and $P_{H_2} = 1.5 \times 10^{-3}$ atm, we find that the pressure of $CH_4$ would be $4 \times 10^8$ atm. In order to have a reasonable pressure of $CH_4$, the partial pressure of $CO_2$ would have to be less than $10^{-8}$ atm, and limestones would not form.

Complete thermodynamic equilibrium could not exist in a reducing atmosphere because of the dependence of the equilibrium proportions of compounds on pressure and hence on altitude. It is more likely that the steady-state concentrations of $CO_2$ and $CH_4$ would be determined not by the equilibrium at sea level but rather by the equilibrium at higher altitude, where the ultraviolet light would provide the activation energy to bring about rapid equilibrium. Under these conditions water would be a gas, and the equilibrium constant would be $10^{20}$, so

$$K_{25} = 10^{20} = P_{CH_4} P^2_{H_2O} / P_{CO_2} P^4_{H_2} = X_{CH_4} X^2_{H_2O} / X_{CO_2} X^4_{H_2} \quad P^{-3}$$

where the $X$'s are the mole fractions and $P$ is the total pressure. If the surface partial pressures were $P_{CH_4} = 1$, $P_{CO_2} = 3.3 \times 10^{-4}$ (the present value), and $P_{H_2} = 1.5 \times 10^{-3}$, the $X$'s would be equal to these partial pressures. We use $X_{H_2O} = 10^{-6}$, which is the present value for $H_2O$ above the tropopause. Equilibrium will be established under these conditions where $P = 2.5 \times 10^{-9}$ atm—the present atmospheric pressure at about 180 km. It is reasonable to assume that equilibrium was established at some high altitude; therefore, carbon dioxide and hydrogen could both have been present at small partial pressures and methane could have been present at a moderate partial pressure in a reducing atmosphere where the pressure of hydrogen was $1.5 \times 10^{-3}$ atm.

Carbon monoxide should not have been an important constituent of the atmosphere, as can be seen from the following reaction

$$CO_2 + H_2 \rightarrow CO + H_2O_{(l)} \qquad K_{25} = 3.2 \times 10^{-4}$$
$$P_{CO}/P_{CO_2} = 3.2 \times 10^{-4} P_{H_2}$$

Using $P_{H_2} = 1.5 \times 10^{-3}$, we have the ratio $P_{CO}/P_{CO_2} = 5 \times 10^{-7}$, which is independent of pressure. Furthermore, carbon monoxide is a relatively reactive compound, and should any significant quantities appear in the atmosphere, it would react rather rapidly to give organic compounds, carbon dioxide and hydrogen, and formate.

Rubey (*8*) and Abelson (*9*) have argued that the surface carbon and nitrogen have come from the outgassing of the interior of the earth instead of from the remaining gases of the cosmic dust cloud from which the earth was formed. The carbon from the outgassing of the earth is a mixture of $CO_2$, CO, and $CH_4$, and hydrogen may be present. While outgassing may have been a significant process on the primitive earth, this does not

mean that the atmosphere was necessarily composed of $CO_2$ and CO. The thermodynamic considerations discussed above would still apply. The carbon dioxide would dissolve in the ocean to form bicarbonate, and $CaCO_3$ would be deposited, and the CO would be unstable, as is demonstrated above.

Many writers quote "authorities" in regard to these questions without understanding what is fact and what is opinion. The thermodynamic properties of C, CO, $CO_2$, $CH_4$, $N_2$, $NH_3$, $O_2$, $H_2O$, and other similar substances are all well known, and the equilibrium mixtures can be calculated for any given composition without question. The only point open to argument is whether equilibrium was approximated or whether a nonequilibrium mixture was present. A mixture of hydrogen and carbon monoxide or hydrogen and carbon dioxide is very unstable at 25°C, but does not explode or react detectably in years. But would such mixtures remain in an atmosphere for millions of years subject to energetic radiation in the high atmosphere? We believe the answer is "No." These mixtures would react even without such radiation in geologic times. Hydrogen and oxygen will remain together at low temperatures for long times without detectable reaction by ordinary methods. The use of radioactive tracers shows that a reaction is proceeding at ordinary temperatures nonetheless.

The buffer systems of the ocean and the calcium silicate–calcium carbonate equilibrium were of sufficient capacity to keep the partial pressure of the carbon dioxide in the atmosphere at a low value; hence, the principal species of carbon in the atmosphere would have been methane, even though the fraction of surface carbon in the oxidation state of carbon dioxide was continuously increasing. This would have been true until the pressure of $H_2$ fell below about $10^{-6}$ atm. It is likely that shortly after this, significant quantities of molecular oxygen would have appeared in the atmosphere.

## Equilibria of Nitrogen Compounds

The equilibrium concentrations of ammonia can be discussed by considering the reaction

$$\frac{1}{2}N_2 + \frac{3}{2}H_2 \rightarrow NH_3 \qquad K_{25} = 7.6 \times 10^5$$

Using $P_{H_2} = 1.5 \times 10^{-3}$, we have $P_{NH_3}/P_{N_2}^{1/2} = 0.04$.

Ammonia is very soluble in water and therefore would displace the above reaction toward the right, giving

$$\frac{1}{2}N_2 + \frac{3}{2}H_2 + H^+ \rightarrow NH_4^+$$

$$(NH_4^+)/P_{N_2}^{1/2}P_{H_2}^{3/2} = 8.0 \times 10^{15}(H^+)$$

which is valid for $p$H's less than 9. At $p$H = 8 and $P_{H_2} = 1.5 \times 10^{-3}$, we have

$$(NH_4^+)/P_{N_2}^{1/2} = 47$$

which shows that most of the ammonia would have been in the ocean instead of in the atmosphere. The ammonia in the ocean would have been largely decomposed when the pressure of hydrogen fell below $10^{-5}$ atm, assuming that the $p$H of the ocean was 8, its present value. A higher $p$H would have made the ammonia less stable; the converse is true for a lower $p$H.

All the oxides of nitrogen would have been unstable and therefore rare. Hydrogen sulfide would have been present in the atmosphere only as a trace constituent because it would have precipitated as ferrous and other sulfides. Sulfur would have been reduced to hydrogen sulfide by the reaction

$$H_2 + S \rightarrow H_2S \qquad K = 6 \times 10^4$$

It is evident that the calculations do not have a quantitative validity because of many uncertainties with respect to temperature, the processes by which equilibrium could be approached, the atmospheric level at which such processes would be effective, and the partial pressure of hydrogen required to provide the necessary rate of escape. In view of these uncertainties, further calculations are unprofitable at the present time. However, we can conclude from this discussion that a reducing atmosphere containing low partial pressures of hydrogen and ammonia and a moderate pressure of methane and nitrogen constitutes a reasonable atmosphere for the primitive earth. That this was the case is not *proved* by our arguments, but we maintain that atmospheres containing large quantities of carbon monoxide and carbon dioxide are not stable and cannot account for the loss of hydrogen from the earth.

Table 1. Present sources of energy averaged over the earth.

| Source | Energy (cal $cm^{-2}$ $yr^{-1}$) |
|---|---|
| Total radiation from sun | 260,000 |
| Ultraviolet light | |
| $\lambda < 2500$ A | 570 |
| $\lambda < 2000$ A | 85 |
| $\lambda < 1500$ A | 3.5* |
| Electric discharges | 4 † |
| Cosmic rays | 0.0015 |
| Radioactivity (to 1.0 km depth) | 0.8‡ |
| Volcanoes | 0.13§ |

* Includes the 1.9 cal $cm^{-2}$ $yr^{-1}$ from the Lyman $\alpha$ at 1216 A (*39*). † Includes 0.9 cal $cm^{-2}$ $yr^{-1}$ from lightning and about 3 cal $cm^{-2}$ $yr^{-1}$ due to corona discharges from pointed objects (*40*). ‡ The value, $4 \times 10^9$ years ago, was 2.8 cal $cm^{-2}$ $yr^{-1}$ (*41*). § Calculated on the assumption of an emission of lava of 1 $km^3$ ($C_p = 0.25$ cal/g, $P = 3.0$ g/$cm^3$) per year at 1000°C.

## Synthesis of Organic Compounds

At the present time the direct or indirect source of free energy for all living organisms is the sunlight utilized by photosynthetic organisms. But before the evolution of photosynthesis other sources of free energy must have been used. It is of interest to consider the sources of such free energy as well as the origin of the appropriate chemical compounds containing excess free energy which supplied the energy for chemical evolution prior to the existence of what should be called living organisms, and before the evolution of photosynthesis.

Table 1 gives a summary of the sources of energy in the terrestrial surface regions. It is evident that sunlight is the principal source of energy, but only a small fraction of this is in the wavelengths below 2000 A which can be absorbed by $CH_4$, $H_2O$, $NH_3$, $CO_2$, and so on. If more complex molecules are formed, the absorption can move to the 2500-A region or to longer wavelengths where a substantial amount of energy is available. With the appearance of porphyrins and other pigments, absorption in the visible spectrum becomes possible.

Although it is probable, it is not certain that the large amount of energy from ultraviolet light would have made the principal contribution to the synthesis of organic compounds. Most of the photochemical reactions at these low wavelengths would have taken place in the upper atmosphere. The compounds so formed would have absorbed at longer wavelengths and therefore might have been decomposed by this ultraviolet light before reaching the oceans. The question is whether the rate of decomposition in the atmosphere was greater or less than the rate of transport to the oceans.

Next in importance as a source of energy are electric discharges, such as lightning and corona discharges from pointed objects, which occur closer to

the earth's surface and hence would have effected more efficient transfer to the oceans.

Cosmic-ray energy is negligible at present, and there is no reason to assume it was greater in the past. The radioactive disintegration of uranium, thorium, and potassium was more important $4.5 \times 10^9$ years ago than it is now, but still the energy was largely expended on the interior of solid materials such as the rocks, and only a very small fraction of the total energy was available in the oceans and atmosphere. Volcanic energy is not only small but its availability is very limited. A continuous source of energy is needed. It contributes little to the evolutionary process to have a lava flow in one part of the earth at one time and to have another flow on the opposite side of the earth years later. For a brief time heat is available at the surface of the lava, but the surface cools and heat flows slowly from the interior for years, making the surface slightly warm. Only a very small contribution to the evolutionary process could be contributed by these energy sources.

## Electric Discharges

While ultraviolet light is a greater source of energy than electric discharges, the greatest progress in the synthesis of organic compounds under primitive conditions has been made with electric discharges. The apparatus used by Miller in these experiments was a closed system of glass, except for tungsten electrodes. The water is boiled in a 500-ml flask which mixes the water vapor and gases in a 5-lit. flask where the spark is located. The products of the discharge are condensed and flow through a U-tube back into the 500-ml flask. The first report (*10*) showed that when methane, ammonia, water, and hydrogen were subjected to a high-frequency spark for a week, milligram quantities of glycine, alanine, and α-amino-*n*-butyric acid were produced.

A more complete analysis (*11, 12*) of the products gave the results shown in Table 2. The compounds in the table account for 15 percent of the carbon added as methane, with the yield of glycine alone being 2.1 percent. Indirect evidence indicated that polyhydroxyl compounds (possibly sugars) were synthesized. These compounds were probably formed from condensations of the formaldehyde that was produced by the electric discharge. The alanine was demonstrated to be racemic, as would be expected in a system which contained no asymmetric reagents. It was shown that the syntheses were not due to bacterial contamination. The addition of ferrous ammonium sulfate did not change the results, and the substitution of $N_2$ for the $NH_3$ changed only the relative yields of the compounds produced.

This experiment has been repeated and confirmed by Abelson (*13*), by Pavlovskaya and Passynsky (*14*), and by Heyns, Walter, and Meyer (*15*). Abelson worked with various mixtures of $H_2$, $CH_4$, CO, $CO_2$, $NH_3$, $N_2$, $H_2O$, and $O_2$. As long as the conditions were reducing conditions—that is, as long as either $H_2$, $CH_4$, CO, or $NH_3$ was present in excess—amino acids were synthesized. The products were the same and the yields as large in many of these mixtures as they were with methane, ammonia, and water. If the conditions were oxidizing, no amino acids were synthesized. These experiments have confirmed the hypothesis that reducing atmospheres are required for the formation of organic compounds in appreciable quantites. However, several of these mixtures of gases are highly unstable. Hence the synthesis of amino acids in these mixtures does not imply that such atmospheres were present on the primitive earth.

Heyns, Walter, and Meyer also performed experiments with different mixtures of gases, with results similar to Abelson's. These workers also used $CH_4$, $NH_3$, $H_2O$, and $H_2S$. They obtained ammonium thiocyanate, thiourea, and thioacetamide as well as compounds formed when $H_2S$ was absent.

The mechanism of synthesis of the amino acids is of interest if we are to extrapolate the results in these simple systems to the primitive earth. Two alternative proposals were made for the synthesis of the amino and hydroxy acids in the spark discharge system. (i) Aldehydes and hydrogen cyanide are synthesized in the gas phase by the spark. These aldehydes and the hydrogen cyanide react in the aqueous phase of the system to give amino and hydroxy nitriles, which are hydrolyzed to amino and hydroxy acids. This mechanism is essentially a Strecker synthesis. (ii) The amino and hydroxy acids are synthesized in the gas phase from the ions and radicals that are produced in the electric discharge.

It was shown that most, if not all, of the amino acids were synthesized according to the first hypothesis, since the rate of production of aldehydes and hydrogen cyanide by the spark and the rate of hydrolysis of the amino nitriles were sufficient to account for the total yield of amino acids (*12*).

This mechanism accounts for the fact that most of the amino acids were α-amino acids, the ones which occur in proteins. The β-alanine was formed not by this mechanism but probably by the addition of ammonia to acrylonitrile (or acrylamide or acrylic acid), followed by hydrolysis to β-alanine.

The experiments on the mechanism of the electric discharge synthesis of amino acids indicate that a special set of conditions or type of electric discharge is not required to obtain amino acids. Any process or combination of processes that yielded both aldehydes and hydrogen cyanide would have contributed to the amount of α-amino acids in the oceans of the primitive earth. Therefore, whether the aldehydes and hydrogen cyanide came from ultraviolet light or from electric discharges is not a fundamental question, since both processes would have contributed to the α-amino acid content. It may be that electric discharges were the principal source of hydrogen cyanide and that ultraviolet light was the principal source of aldehydes, and that the two processes complemented each other.

## Ultraviolet Light

It is clear from Table 1 that the greatest source of energy would be ultraviolet light. The effective wavelengths would be $CH_4 < 1450$ A, $H_2O < 1850$ A, $NH_3 < 2250$ A, $CO < 1545$ A, $CO_2 < 1690$ A, $N_2 < 1100$ A, and $H_2 < 900$ A. It is more difficult to work with ultraviolet light than with electric discharges because of the small wavelengths involved.

The action of the 1849-A Hg line on a mixture of methane, ammonia, water, and hydrogen produced only a very small yield of amino acids (*16*). Only $NH_3$ and $H_2O$ absorb at this wavelength, but apparently the radical reactions formed active carbon intermediates. The limiting factor seemed to be the synthesis of hydrogen cyanide. Groth (*17*) found that no amino acids were produced by the 1849-A line of mercury with a mixture of methane, ammonia, and water, but that amines and amino acids were formed when the 1470-A and

1295-A lines of xenon were used. The 1849-A line produced amines and amino acids with a mixture of ethane, ammonia, and water. The mechanism of this synthesis was not determined. Terenin (*18*) has also obtained amino acids by the action of the xenon lines on methane, ammonia, and water.

We can expect that a considerable amount of ultraviolet light of wavelengths greater than 2000 A would be absorbed in the oceans, even though there would be considerable absorption of this radiation by the small quantities of organic compounds in the atmosphere. Only a few experiments have been performed which simulate these conditions.

In a most promising experiment, Ellenbogen (*19*) used a suspension of ferrous sulfide in aqueous ammonium chloride through which methane was bubbled. The action of ultraviolet light from a mercury lamp gave small quantities of a substance with peptide frequencies in the infrared. Paper chromatography of a hydrolyzate of this substance gave a number of spots with Ninhydrin, of which phenylalanine, methionine, and valine were tentatively identified.

Bahadur (*20*) has reported the synthesis of serine, aspartic acid, asparagine, and several other amino acids by the action of sunlight on paraformaldehyde solutions containing ferric chloride and nitrate or ammonia. Pavlovskaya and Passynsky (*21*) have also synthesized a number of amino acids by the action of ultraviolet light on a 2.5-percent solution of formaldehyde containing ammonium chloride or nitrate. These high concentrations of formaldehyde would not have occurred on the primitive earth. It would be interesting to see if similar results could be obtained with $10^{-4}M$ or $10^{-5}M$ formaldehyde. This type of experiment deserves further investigation.

## Radioactivity and Cosmic Rays

Because of the small amount of energy available, it is highly unlikely that high-energy radiation could have been very important in the synthesis of organic compounds on the primitive earth. However, a good deal of work has been done in which this type of energy has been used, and some of it has been interpreted as bearing on the problem of the origin of life.

Dose and Rajewsky (*22*) produced amines and amino acids through the action of x-rays on various mixtures of $CH_4$, $CO_2$, $NH_3$, $N_2$, $H_2O$, and $H_2$. A small yield of amino acids was obtained through the action of 2 Mev electrons on a mixture of $CH_4$, $NH_3$, and $H_2O$ (*23*).

Table 2. Yields from sparking a mixture of $CH_4$, $NH_3$, $H_2O$, and $H_2$; 710 mg of carbon was added as $CH_4$.

| Compound | Yield [moles ($\times 10^5$)] |
|---|---|
| Glycine | 63. |
| Glycolic acid | 56. |
| Sarcosine | 5. |
| Alanine | 34. |
| Lactic acid | 31. |
| N-Methylalanine | 1. |
| α-Amino-*n*-butyric acid | 5. |
| α-Aminoisobutyric acid | 0.1 |
| α-Hydroxybutyric acid | 5. |
| β-Alanine | 15. |
| Succinic acid | 4. |
| Aspartic acid | 0.4 |
| Glutamic acid | 0.6 |
| Iminodiacetic acid | 5.5 |
| Iminoacetic-propionic acid | 1.5 |
| Formic acid | 233. |
| Acetic acid | 15. |
| Propionic acid | 13. |
| Urea | 2.0 |
| N-Methyl urea | 1.5 |

The formation of formic acid and formaldehyde from carbon dioxide and water by 40 Mev helium ions was mentioned previously. These experiments were extended by using aqueous formic acid (*24*). The yield per ion pair was only $6 \times 10^{-4}$ for formaldehyde and 0.03 for oxalic acid. Higher yields of oxalic acid were obtained from $Ca(HCO_3)_2$ and $NH_4HCO_3$ by Hasselstrom and Henry (*25*). The helium ion irradiation of aqueous acetic acid solutions gave succinic and tricarbolic acid along with some malonic, malic, and citric acids (*26*).

The irradiation of 0.1- and 0.25-percent aqueous ammonium acetate by 2 Mev electrons gave glycine and aspartic acid (*27*). The yields were very small. Massive doses of gamma rays on solid ammonium carbonate yielded formic acid and very small quantities of glycine and possibly some alanine (*28*).

The concentrations of carbon compounds and the dose rates used in these experiments are, in all probability, very much larger than could be expected on the primitive earth, and the products and yields may depend markedly on these factors, as well as on the effect of radical scavengers such as $HS^-$ and $Fe^{2+}$. It is difficult to exclude high-energy radiations entirely, but if one is to make any interpretations from laboratory work, the experiments should be performed with much lower dose rates and concentrations of carbon sources.

## Thermal Energy

The older theories of the formation of the earth involved a molten earth during its formation and early stages. These theories have been largely abandoned, since the available evidence indicates that the solar system was formed from a cold cloud of cosmic dust. The mechanisms for heating the earth are the gravitational energy released during the condensation of the dust to form the earth and the energy released from the decay of the radioactive elements. It is not known whether the earth was molten at any period during its formation, but it is clear that the crust of the earth would not have remained molten for any length of time.

Studies on the concentration of some elements in the crust of the earth indicate that the temperature was less than 150°C during this lengthy fractionation, and that it was probably close to present terrestrial temperatures (*29*).

Fox (*30*) has maintained that organic compounds were synthesized on the earth by heat. When heated to 150°C, malic acid and urea were converted to aspartic acid and ureidosuccinic acid, and some of the aspartic acid was decarboxylated to α- and β-alanine. The difficulty with these experiments is the source of the malic acid and urea on the primitive earth—a question not discussed by Fox. Fox has also synthesized peptides by the well-known reaction (*31*) of heating amino acids at 150° to 180°C, and the yield of peptides has been increased by using an excess of aspartic or glutamic acid (*32*). There is a difficulty connected with heating amino acids and other organic compounds to high temperatures. Geological conditions can heat amino acids to temperatures above 100°C over long periods of time, but it is not likely that this could occur over short periods. Abelson (*33*) has shown that alanine, one of the more stable amino acids, decarboxylates to methylamine and carbon dioxide. The mean life of alanine is $10^{11}$ years at 25°C but only 30 years at 150°C. Therefore, any extensive heating

of amino acids will result in their destruction, and the same is true for most organic compounds. In the light of this, and since the surface of the primitive earth was probably cool, it is difficult to see how the processes advocated by Fox could have been important in the synthesis of organic compounds.

## Surface Reactions, Organic Phosphates, and Porphyrins

It is likely that many reactions were catalyzed by adsorption on clay and mineral surfaces. An example is the polymerization of aminoacetonitrile to glycine peptides in the presence of acid clays, by Akabori and his co-workers (*34*). Formaldehyde and acetaldehyde were shown to react with polyglycine adsorbed on kaolinite to give serine and threonine peptides. This field offers many possibilities for research.

Gulick (*35*) has pointed out that the synthesis of organic phosphates presents a difficult problem because phosphate precipitates as calcium and other phosphates under present earth conditions, and that the scarcity of phosphate often limits the growth of plants, especially in the oceans. He proposes that the presence of hypophosphites, which are more soluble, would account for higher concentrations of phosphorus compounds when the atmosphere was reducing. Thermodynamic calculations show that *all* lower oxidation states of phosphorus are unstable under the pressures of hydrogen assumed in this article. It is possible that stronger reducing agents than hydrogen reduced the phosphate or that some process other than reduction solubilized the calcium phosphate. This problem deserves careful attention.

The synthesis of porphyrins is considered by many authors to be a necessary step for the origin of life. Porphyrins are not necessary for living processes if the organism obtains its energy requirements from fermentation of sugars or other energy-yielding organic reactions. According to the heterotrophic theory of the origin of life, the first organisms would derive their energy requirements from fermentations. The metabolism of sulfate, iron, $N_2$, hydrogen, and oxygen appears to require porphyrins as well as photosynthesis. Therefore, porphyrins probably would have to be synthesized before free energy could be derived from these compounds. While porphyrins may have been present in the environment before life arose, this is apparently not a necessity, and porphyrins may have arisen during the evolution of primitive organisms.

## Intermediate Stages in Chemical Evolution

The major problems remaining for an understanding of the origin of life are (i) the synthesis of peptides, (ii) the synthesis of purines and pyrimidines, (iii) a mechanism by which "high-energy" phosphate or other types of bonds could be synthesized continuously, (iv) the synthesis of nucleotides and polynucleotides, (v) the synthesis of polypeptides with catalytic activity (enzymes), and (vi) the development of polynucleotides and the associated enzymes which are capable of self-duplication.

This list of problems is based on the assumption that the first living organisms were similar in chemical composition and metabolism to the simplest living organisms still on the earth. That this may not be so is obvious, but the hypothesis of similarity allows us to perform experiments to test it. The surprisingly large yields of aliphatic, hydroxy, and amino acids—α-amino acids rather than the other isomers—in the electric-discharge experiments, plus the arguments that such syntheses would have been effective on the primitive earth, offer support for this hypothesis. Further support can be obtained by demonstrating mechanisms by which other types of biologically important compounds could be synthesized.

Oparin (*1*) does not view the first organism as a polynucleotide capable of self-duplication but, rather, as a coacervate colloid which accumulates proteins and other compounds from the environment, grows in size, and then splits into two or more fragments, which repeat the process. The coacervate would presumably develop the ability to split into fragments which are very similar in composition and structure, and eventually a genetic apparatus would be incorporated which would make very accurate duplicates.

These two hypotheses for the steps in the formation of the first living organism differ mainly in whether the duplication first involved the relatively accurate duplication of nucleic acids, followed by the development of cytoplasm duplication, or whether the steps occurred in the reverse order. Other sequences could be enumerated, but it is far too early to discuss profitably the exact nature of the first living organism.

It was probably necessary for the primitive organisms to concentrate organic and inorganic nutrients from their environment. This could be accomplished by means of a membrane or by absorption on rocks or clays (*36*). The development of optical activity in living organisms is another important problem. This has been discussed by many authors and is not taken up here.

## Life on Other Planets

Life as we know it—and we know of no other variety of life than that existing on the earth—requires the presence of water for its chemical processes. We know enough about the chemistry of other systems, such as those of silicon, ammonia, and hydrogen fluoride, to realize that no highly complex system of chemical reactions similar to that which we call "living" would be possible in such media. Also, much living matter exists and grows actively on the earth in the absence of oxygen, so oxygen is *not* necessary for life, although the contrary is often stated. Moreover, the protecting layer of ozone in the earth's atmosphere is not necessary for life, since ultraviolet light does not penetrate deeply into natural waters and also because many carbon compounds capable of absorbing the ultraviolet light would be present in a reducing atmosphere.

It is possible for life to exist on the earth and to grow actively at temperatures ranging from 0°C, or perhaps a little lower, to about 70°C. It seems likely that if hot springs were not so temporary, many plants and possibly animals would evolve which could live in such temperatures. Plants are able to produce and accumulate substances which lower the freezing point of water, and hence they can live at temperatures below 0°C. At much lower temperatures the reactions would probably be too slow to proceed in reasonable periods of time. At temperatures much above 120°C, reaction velocities would probably be so great that the nicely balanced reactions characteristic of living things would be impossible. In addition, it is doubtful whether the organic polymers necessary for living organisms would be stable much above 120°C; this is prob-

ably true even when allowance is made for the amazing stability of the enzymes of thermophilic bacteria and algae.

Only Mars, Earth, and Venus conform to the general requirements so far as temperatures are concerned. Mars is known to be very cold and Venus may be too hot. Observations of the blackbody emission of radio waves from Venus indicate surface temperatures of 290° to 350°C (*37*). The clouds of Venus have the polarization of water droplets. Clearing of the clouds occurs, and this indicates that the clouds are composed of some volatile substance, for nonvolatile dust could hardly settle out locally. However, no infrared bands of water have been observed. It is possible that this is due to a very dry, high atmosphere, such as is characteristic of the earth, and to a cloud level that rises to very near the tropopause, so that there is little water vapor above the reflecting layer.

Mars is known to be very cold, with surface temperatures of +30°C to −60°C during the day. The colors of Mars have been observed for many years by many people. The planet exhibits seasonal changes in color—green or bluish in the spring and brown and reddish in the autumn. Sinton (*38*) has observed an absorption at 3.5 μ in the reflected light of Mars. This corresponds to the C-H stretching frequency of most organic compounds, but many inorganic compounds have absorptions at this wavelength. The changing colors of Mars and the 3.5 μ absorption are the best evidence, however poor it may be, for the existence of life on the planet. One thing that can be stated with confidence is that if life exists there, then liquid water must have been present on the planet in the past, since it is difficult to believe that life could have evolved in its absence. If this was so, water must have escaped from the planet, as very little water remains there now and no liquid water has been observed. Hence, oxygen atoms must escape from the planet. This is possible if the high atmosphere has a temperature of 2000°K, and this may well be the case in view of the high temperatures in the high atmosphere of the earth.

Surely one of the most marvelous feats of 20th-century science would be the firm proof that life exists on another planet. All the projected space flights and the high costs of such developments would be fully justified if they were able to establish the existence of life on either Mars or Venus. In that case, the thesis that life develops spontaneously when the conditions are favorable would be far more firmly established, and our whole view of the problem of the origin of life would be confirmed (*42*).

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# Soviet Industrial Growth

## Estimates of it face difficulties, but the "best" Western guess finds Soviet claims much exaggerated.

G. Warren Nutter

In applying Lord Kelvin's famous dictum to the workaday habits of many economists, one of my revered teachers, Frank H. Knight, used to say: "If you can't measure, measure anyhow." The student of the Soviet economy cannot avoid getting caught on the barb of this remark. Knowing the formidable difficulties of measuring, in a simple and direct way, such a complex and elusive thing as Soviet economic growth, he might refuse the job. But the public will not be denied: the question is too pressing to be left unanswered. The student is thus forced to act on a second principle also often enunciated by Knight, one that he attributes in turn to a revered teacher of his own: no question can be called unanswerable if it has to be answered, and some answers are better than others—or, at least, some are worse.

The question before us is how fast Soviet industry has been growing. It has been answered many times, and there is no doubt that some answers are worse than others. Within the brief compass of this article, I will try to explain why this is so and to give my own answers, without presuming that they are best.

### The Worst Answer

The worst answer, almost all Western scholars agree, comes in the form of the official Soviet index of industrial production. According to that index, industrial output has multiplied 36 times since 1913 in the Soviet Union, or 7 times more than in the United States, where it has multiplied around 5 times. The myth is easily dispelled: since Russian industry produced about a seventh as much as American industry in 1913, its output would now exceed the American level if the official Soviet index were correct. Not even the Soviet leaders claim this. In a recent speech, Khrushchev stated that Soviet industrial output had reached half the American level, a figure that is also probably too high. This would seem to imply a contradiction in Soviet arithmetic, a rare occurrence. The contradiction is avoided by an official statement that Russian output in 1913 was only 7 percent of the American level, a fancifully low estimate.

Unfortunately, the defects in the Soviet index cannot be fully analyzed and corrected because the underlying details have not been published. We do know that the index is not constructed in accord with generally accepted Western practices and that some of the methods used are bound to exaggerate growth. For example, until recent years output was evaluated in so-called "1926/27 prices," but new products were added to the index whenever they first came into production. They were supposed to be evaluated in the prices they would have had in 1926/27, had they been produced then. But this is a bit difficult to do in the case of television sets, jet airplanes, rockets, and the like. The expedient actually followed was to price each new product at its initial cost of production, which was inflated on two counts. First, initial costs are abnormally high, since they include developmental expenses, apply to a pilot rate of production, and do not allow for normally rapid reductions in cost, attributable to learning. Second, there was a steady and sharp inflation in the general price level over this period. Hence new products, which tend to grow faster in output than older ones, were entered into the index periodically at heavily inflated weights.

Other practices have a similar effect in exaggerating growth. Soviet statisticians seem to be aware of the more serious defects in the official index, and improvements have been made in the postwar years. Nevertheless, the index continues to exaggerate growth, though not as heavily as before. Meanwhile, the index for earlier years, having become firmly established in official dogma, stands little chance of being revised.

### The Thorny Path to Better Answers

The only alternative open to the Western scholar is to start from scratch and construct his own production index. Enormous problems are immediately raised. In the first place, the basic data on physical output of individual industries also come from official sources; and, though they are not as unreliable as broader index numbers, they have many shortcomings. In the second place, the turbulent and uneven nature of Soviet industrial growth creates measurement troubles that could not be eliminated even if the basic data were ideal.

Social scientists accustomed to doing empirical work may find it hard to believe that Soviet statistics are "really" worse than others, because every empirical researcher in no matter what field of the social sciences quickly becomes convinced, for rather good reasons, that no data could be as bad as those he is forced to work with. Why call the kettle black when it is probably no grayer than the pot?

Let us acknowledge at once that all statistics contain faults and errors. Let us also acknowledge that no government or other agency is free from the temptation to stretch figures to its own account, if it feels it can get away with it. Representative government, competitive scholarship, and free public discourse are the Western institutions that have counteracted error and misrepresentation in statistics, imperfectly to be sure, but at least to some degree.

The peculiar difficulties with Soviet statistics stem, in the first instance, from the system of authoritarian, centralized planning—from what has been called a "command economy." Published statistics come from only one source, the state. There are no independent sources to restrain each other or to be used as checks against each other, except to the extent that related figures published by different state agencies might be uncoordinated before publication. On the other side, the suppliers of data to the central authorities—the economic enterprises and other administrative units—have a stake in the figures they report,

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since their performance is judged on the basis of them. The Soviet statistical authorities do not hide their concern over the misreporting that results from this feature of the economic system.

A second set of difficulties stems from the crusading nature of Soviet communism. Statistics are grist for the propaganda mill. Knowing the ideological views of Soviet leaders, one cannot expect them to dispense facts in a passive and detached manner.

For both broad reasons, Soviet statistics are selective and of varying reliability and ambiguity. The policy of selectivity has two rather opposing results as far as statistics on physical output are concerned. On the one hand, some areas of poor performance are shielded from view, being underrepresented in published data. On the other hand, some of the more rapidly expanding economic activities associated with the military sector are also not reported on. It is impossible to determine the net bias of the sample of published data: whether there is, on this count, a net over- or understatement of growth (*1*).

A few broad generalizations can be made about the reliability of the published statistics. In the first place, absolute output is probably overstated in the case of most industries, particularly for the years within the Plan period, though the degree of overstatement cannot be determined. In the second place, growth in output is also probably overstated relative to a prerevolutionary base, but not necessarily over other parts of the Soviet period. Over some of the latter years, growth may be overstated, over others understated, and over still others more or less accurately reported. This will vary from industry to industry and from one situation to another.

Let us now turn briefly to some of the more technical problems involved in constructing production indexes for Soviet industry. A production index is a synthetic measure that translates diverse growth rates for many different products into the single hypothetical rate that presumably would have obtained if, in fact, all products had grown at the same rate. The translation is accomplished by, in effect, transforming the many different products into a common generalized product—by weighting each product by its relative cost of production in a particular year. Thus, swords are "beaten" into plowshares by expressing both in terms of their productive value: the output of each is multiplied (weighted) by its unit cost of production, and the resulting values are added together to get aggregate production. The aggregate can be thought of as the hypothetical quantity of plowshares (or swords) that could be produced if they were the only things produced. Growth between one year and another is measured by the ratio of aggregate production in the later to the earlier year, outputs of swords and plowshares being weighted in both years by the costs in a single specified year.

Table 1. Average ann growth rates of Soviet industrial production according to different production indexe ercentages).

| Index | Unadjusted | | Adjusted* | |
|---|---|---|---|---|
| | 1913–1955 | 1928–1955 | 1913–1955 | 1928–1955 |
| Calculated indexes | | | | |
| All indexes | 3.0–4.7 | 5.1–7.3 | 2.7–4.4 | 4.7–6.9 |
| "Best" index | 4.2 | 6.5 | 3.9 | 6.1 |
| Official Soviet index | 8.2 | 11.9 | 7.9 | 11.5 |

* Adjusted to exclude gains from territorial expansion during World War II.

Even under conditions in which the basic data needed to compute index numbers are close to ideal, the numbers can be fickle. Their vagaries have made necessary the development of a specialized theory of index numbers, whose mysteries cannot be explored here. We must be satisfied to note that the numerical value of a production index will depend on such things as the weighting formula used, the year chosen for the weights, the structure of production, the path of growth followed, and so on. The divergence between indexes constructed in alternative ways tends to be greatest when an economy is undergoing swift and radical changes in its structure and when growth rates for different industries are widely dispersed.

Soviet industry has undergone just such swift and radical changes, particularly during the late 1920's and early 1930's. Growth rates have also diverged widely from one sector of industry to another, being much higher in "heavy" industry than in consumer goods industries. Growth has been interrupted at critical points by major disturbances. Quantitative growth has not been accompanied by the general improvement in quality that has characterized industrial development in most Western countries. These difficulties of measurement are compounded by the fact that there are few reliable data on costs of production: as is now acknowledged by Soviet economists, the Soviet price system does not accurately reflect relative costs of production. These factors, coupled with the questionable reliability of statistics on physical output, makes the calculation of production indexes for Soviet industry unusually treacherous.

## Some Better Answers

In the face of these many problems, the only satisfactory approach is to measure Soviet industrial growth in a number of ways and examine the converging lines of evidence. In the work I have been associated with at the National Bureau of Economic Research (*2*), we have constructed a variety of production indexes, measuring productive activity at three different stages of fabrication, using Soviet weights for three different years (1913, 1928, and 1955) and American weights for four others (1914, 1929, 1939, and 1954), and varying in extensiveness of product coverage. These indexes have been supplemented by studies of the performance of individual industries, summarized by various techniques, including liberal comparisons with performance of counterpart industries over relevant periods of American industrial history. Since the study has not been completed, the figures presented here should be viewed as tentative and subject to revision.

Let us look first at the production indexes, concentrating our attention on the growth they show over two periods, 1913–1955 and 1928–1955. The first encompasses the whole Soviet era, except for the last few years; the second, the period under comprehensive centralized planning.

Over the longer Soviet period, these indexes show industrial production as multiplying between 3.5 and 6.8 times; over the Plan period, between 3.8 and 6.6 times. If we choose from among these indexes a single one that conforms most closely in its construction to the kind of production index preferred in the West and treat that index as the "best" estimate of Soviet industrial

Table 2. Comparisons of average annual growth rate for industry in the United States and Soviet Union (percentages).

| Period | Soviet Union* | United States |
|---|---|---|
| 1913–1955 | 3.9 | 3.7 |
| 1885–1927 | | 4.8 |
| 1877–1919 | | 5.0 |
| 1928–1955 | 6.1 | 3.7 |
| 1885–1912 | | 5.3 |
| 1877–1904 | | 5.6 |

* Adjusted to exclude gains from territorial expansion during World War II.

growth, we find that Soviet industrial output multiplied 5.6 times over the entire Soviet period and 5.5 times over the Plan period. From these indexes, we note the failure of Soviet industry to show any net growth between 1913 and 1928. According to the official Soviet index, the corresponding multiples are 27 times for 1913–1955 and 21 times for 1928–1955. Hence, if the "best" estimate is accepted as correct, the official Soviet index contains a four- to fivefold exaggeration of growth over the periods under consideration.

For comparative purposes, it is advisable to translate these multiples of growth into average annual rates (see Table 1). The "best" estimate of growth is shown to be 4.2 percent a year, on the average, over the entire Soviet period and 6.5 percent a year over the Plan period, rates that are somewhat less than half those shown by the official Soviet index (*3*). Some of this growth is attributable to the acquisition of territory during and after World War II: the Baltic States, about half of Poland, a part of Rumania, and other lesser territories. These regions have added about 10 percent to Soviet industrial production, and eliminating this gain reduces the average annual growth rate to 3.9 percent for the entire Soviet period and to 6.1 percent for the Plan period.

These general results may be checked in two broad ways. First, study of about 50 individual industries indicates that Russian output in 1913 (within the pre-1939 Soviet territory) and 1955 was, on the average, roughly the same as U.S. output in 1885 and 1920, respectively. This would mean that Soviet industrial production grew approximately as much between 1913 and 1955 as American production grew between 1885 and 1920. Over the latter period, American industrial production multiplied 5.4 times, almost the same figure as found by the "best" index for Soviet industry for 1913–1955.

Second, a similar comparison can be made for aggregate production. If the output of Soviet industrial products is evaluated in American prices, we find—after making allowance for the degree of coverage represented by the products for which this evaluation can be made—that the net production of Soviet industry was about 15 percent as large as net production of American industry in 1913, and about 22 percent as large in 1955 (*4*). The estimate for 1955 is probably less reliable than the one for 1913, and may be in error by as much as 10 percent in either direction. Looking back into American industrial history, we find that in 1877 production was about 15 percent of the level in 1913, and in 1913 it was about 22 percent of the level in 1955. Hence, through this indirect path, we would suppose that Soviet industrial output in 1913 and 1955 corresponded roughly with American industrial output in 1877 and 1913, respectively. Between 1877 and 1913, American industrial output multiplied 6.7 times, which is roughly the multiple shown for Soviet industry for 1913–1955 by the fastest-growing index constructed in our work at the National Bureau of Economic Research.

## The Answers Put into Perspective

The estimates of Soviet industrial growth presented here will take on more meaning if they are compared with the pace of industrial growth in this country. We note that Soviet growth has been slightly faster than American growth for 1913–1955, and much faster for 1928–1955 (see Table 2). It would be prudent to suppose that the differential will continue in favor of the Soviet Union over the near future.

These comparisons are important for many purposes, but they do not indicate the relative growth-generating capacities of the Soviet and American economic systems, even as far as industry alone is concerned. The industrial potentials of the two economies are similar: they are both large and richly endowed with natural resources. Given the state of the industrial arts, the rate of industrial growth tends to depend on the level of productive activity relative to the industrial potential. The lower that level, the faster the growth rate tends to be. It is therefore useful to compare Soviet with American growth over periods in which industrial production started at roughly the same level. In this respect, the American periods 1885–1920 or 1877–1919 are more or less comparable with the Soviet period 1913–1955, and 1885–1912 or 1877–1904 with 1928–1955.

Over the longer "comparable" periods, the American growth rate exceeded the Soviet one; over the shorter ones, the reverse is true (see Table 2). The growth-generating superiority of one industrial system over the other—as far as it is revealed by these rather mechanical comparisons—remains in doubt, to be resolved only by the future course of history. It should be noted, however, that this set of comparisons involves a substantial advantage in favor of the Soviet Union, since it has had 20th-century technology at its disposal in working out its course of industrialization.

## Warnings about Conclusions

Since the aims of this brief survey of a particular measurement problem have been modest, it would be a mistake to conclude too much from it. Our attention has been focused on the broad aggregate of industrial production, not on the segments of industry receiving highest priority from Soviet leaders. Those are the segments that promote state power, in particular military power. It is wrong to infer military power from general industrial strength, and vice versa.

One must also be careful not to make simple mechanical comparisons between industrial growth rates in the Soviet Union and the West. The character of industrial growth and the context within which it has taken place differ so materially in the two parts of the world that comparisons of highly generalized measures of industrial growth carry a very limited meaning. In the Soviet Union enhancement of state power has been the primary objective of economic policy, the consumer being treated as a residual claimant. Heavy industry and ordnance have been supported at the expense of consumer goods; and other important sectors of the economy—agriculture, construction, and consumer services—have been neglected in favor of industrial growth. Leisure has grown very slowly, and human losses that stagger the imagination have been a part of the cost of growth. These matters are not brought up to place the actual quantitative achievements in a shadow; those achievements are real enough. But a full appraisal of industrial performance and its significance requires that it be viewed against the broader background of economic and social achievements as a whole.

**References and Notes**

1. These brief comments apply to the condition of economic statistics since 1956. Between 1938 and 1956, statistics on the physical output of individual industries were not published at all in the Soviet Union, with a few minor exceptions.
2. The National Bureau of Economic Research is a nonprofit organization engaged in economic research, with its main offices in New York. Arthur F. Burns of Columbia University is president, and Solomon Fabricant of New York University is director of research.
3. The phenomenon of compounding makes the average annual growth rates diverge less percentagewise than the multiples of growth for the longer spans.
4. The general opinion of American specialists in Soviet studies seems to be that Soviet industrial production was about a third of the American level in 1955, which is considerably higher than the estimate given here [see, for example, *Soviet Economic Growth: A Comparison with the United States* (Joint Economic Committee, Washington, D.C., 1957), p. 11]. I can only say that I have not been able to reproduce the conventional estimate by direct calculations.

# Walter D. Bonner, Scholar, Chemist, Gifted Teacher

Walter Daniel Bonner, late professor and head of chemistry at the University of Utah at Salt Lake City, was born on 27 October 1878 in the town of Osceola, Nebraska. He was the oldest of eight children of a father of Scottish-American descent (via Pennsylvania and Ohio) who had been educated for the Presbyterian ministry. The father, wearied by a three-year diet of Greek and Latin, turned instead to cabinet-making and carpentry, at which he was adept. Bonner's mother was of New England ancestry and was relatively unschooled but could play the piano and sing with real ability. When young Walter was nine years old, his parents moved permanently to a farm, a step which never proved financially rewarding. From the age of nine until he reached his majority, Walter worked on this farm or hired out as a farm hand, and during the winter months he attended small country schools. It may be said that he was influenced on the side of scholarly pursuits and manual skills, mainly by his father, and in his love for and ability in music by his mother. The Bonner ancestry is liberally sprinkled with competent millers and engineers.

Because of the economic necessity for doing farm work, Walter was unable to begin high school until he was 21, when he entered Nebraska Wesleyan University and its associated preparatory school. Being compelled to support himself at Wesleyan, he did janitor work, at night, and carpentering, at which he excelled, during the summer vacations. His food during school terms often consisted of cooked oatmeal only. Stimulation to take up chemistry came from a young professor, F. J. Alway, who had studied organic chemistry in Germany. With Alway, Bonner published three papers on organic chemistry; he was elected to Phi Beta Kappa and received the B.S. degree in 1906. At Wesleyan University he met Miss Grace Gaylord, also a student of chemistry, whom he married in 1909.

Persuaded by Alway, Bonner applied for and received a fellowship at Princeton where he studied under G. A. Hulett who, being of a more modern and farsighted temperament, soon convinced Bonner that physical chemistry was the more fundamental subject to pursue. With Hulett he published his now classic quantitative work on constant boiling hydrochloric acid solutions. The powers that be at Princeton were allegedly displeased by Bonner's change to physical chemistry, and it became apparent to both him and Hulett that he should settle for an M.A. in 1908 and go on to the University of Toronto, Canada. There he worked under the already prominent Professor W. Lash Miller on phase rule studies and received his Ph.D. in 1911, as well as appointments as lecturer and then assistant professor at Queens University.

Walter Bonner was born, raised, and educated and he worked in a comparatively serene but remarkably significant period in the history of science and mathematics, and he was acutely aware of this. The physical sciences and mathematics advanced amazingly along quantitative and fundamental paths as well as in depth and breadth of philosophical understanding.

Such was the background and spirit of science when Professor Bonner was asked to join the faculty of the University of Utah and assume the headship of the chemistry department in 1915. There was one other member of the chemistry staff, newly appointed Professor Elton Quinn; later Thomas B. Brighton and still later a professor of physics, Orin Tugman, were appointed. Work leading to the bachelor's degree could be given, but at first no further. The science building was attractive and rugged but the research facilities were meager. Both Bonner and Quinn were artists at glass blowing, and they made apparatus, did the teaching, and encouraged the undergraduates to embark on such researches as were then possible. There was a gentlemanly but stern dean (and professor of mathematics) who resisted all efforts to encourage research and advanced graduate work at the University, but through Bonner's patient insistence, and some delphic magic, more and more research on the part of seniors and master's candidates was done.

The stipends of the early graduate assistants were painfully small, as this writer can testify, and on numerous occasions Mrs. Bonner graciously provided hospitality. The monumental result of Bonner's more than 30 years of teaching, research, and encouragement was, besides many significant scientific papers and a book, over a hundred men who went on to advanced graduate work in chemistry and physics at other universities and colleges, and some two hundred more who went into medicine. Largely through his efforts such scholars as A. A. Noyes of Pasadena, Joel Hildebrand and G. N. Lewis of Berkeley, and others, lectured on occasions at the University. He never had a secretary.

Bonner was a man of even temper, warmth, and pleasant persistence. He was versed in literature of both ancient and modern origin, and well informed on current events and their significance. Though never sanctimonious or formally religious, he could quote the Holy Writ to good effect when he deemed it appropriate. His family counted six sons, all of whom went on to advanced degrees in the physical or biological sciences, and one daughter.

Here then is the account of a scholarly man and notable teacher who did splendidly, often under extremely adverse circumstances. He passed away on 4 January 1956 in New Haven, Connecticut.

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# News of Science

## President and Congress Still at Loggerheads on Education

On 7 July President Eisenhower vetoed the $1.4 billion omnibus housing bill (S.57), declaring it to be "so excessive in the spending it proposes, and so defective in other respects, that it would do far more damage than good." Among the provisons of the vetoed bill was a loan fund of $62.5 million to cover construction or rehabilitation of college classrooms and laboratories and purchase of new equipment.

The veto, according to observers, reflects the impasse which Congress and the Administration have reached over the government's role in education. The result, despite attempts at accommodation on the part of congressional leaders, is inaction.

The Administration wants a bill that will help colleges secure loans from private sources. The vetoed bill provided for the financing of the new program through the familiar practice of authorizing colleges to borrow funds for housing construction from the government at federal interest rates. This, the Administration feels, would be less sound fiscally than its own proposal.

Under the Administration proposal, the colleges would sell bonds to private investors to raise funds for construction. The government would guarantee payment of interest and principal on the bonds and would provide debt-retirement assistance in the form of commitments to pay 25 percent of the principal on long-term bonds issued by the colleges to finance construction of science and engineering facilities. Only such a bill, says Arthur Flemming, Secretary of Health, Education, and Welfare, has a chance of presidential approval.

### Aid for Local Schools

Differences in administration and congressional thinking are evident in other areas of education as well. In the area of public school support, Congress has come up with the $4.4 billion Murray-Metcalf bill, which would authorize outright grants to the schools for 4 years on a "nonmatching" basis, while the Administration proposes that the government help needy school districts amortize bonds issued locally by the districts. Again, according to observers, the fundamental issue behind the education controversy is fiscal. Should the government *give* to help schools, and thereby spend to a degree the current Administration believes dangerous, or should it *facilitate* fund-raising action by school districts?

In the mind of administration officials such as Flemming, there is no question but that the second alternative is the better one. Faced with this conviction, congressional leaders such as Lyndon Johnson, trying to get some action on the critical education programs, have trimmed fund requests in an attempt to placate the President. To date, this kind of accommodation has resulted in considerable loss to the congressional leadership. Bills on which concessions have been made have not been signed; the congressional leaders, particularly Johnson and Rayburn, have come under attack by Democratic spokesmen such as Paul Butler, National Democratic Committee chairman.

The only possible eventual gain for the Democrats lies in the value of administration rebuffs as campaign issues in the coming election. Democrats can say that, despite their efforts to meet the Administration's requests, their "reasonable" proposals were turned down. Thus, if a paralysis on important legislation results from the conflict between a Republican president and a Democratic congress, the responsibility for it, the Democrats can claim, rests with the President.

### Defense Education Act

If the new education programs are running aground in the storm between the Administration and the Congress, it can be said at least that an established program, set up under the National Defense Education Act of 1958, is proceeding well. The various provisions of this act, which was generally regarded last year as a foot-in-the-door for federal aid to education (critics said it was the camel's nose under the tent for federal control of education) are now being implemented. Recently, President Eisenhower signed a supplemental appropriation bill which allocates $4.5 million for graduate fellowships under the 1958 act. This action brought to a total of 1000 the number of fellowships authorized for the first year of the program. The graduate fellowships are designed to increase the supply of college teachers and scholars and to expand graduate education in all fields. Under the provisions of the NDEA, 1500 additional fellowships may be granted for each of the three academic years following the one that begins in September. The fellowships, which run for 3 years, carry stipends of $2000, $2200, and $2400 for the first, second, and third years of study, respectively. Under the act, applicants who plan to go into teaching must be given preference.

In the same bill, appropriation of $30.5 million for student loans was authorized. Under this provision of the National Defense Education Act, undergraduate students can borrow up to $1000 a year to defray the costs of education. One year after the student has completed full-time study, he must begin repaying the loan and the 3 percent interest on it. Student borrowers who, on graduating, teach in public elementary and secondary schools will be eligible for cancellation of up to 50 percent of the loan if they teach full-time for at least 5 years. As in the fellowship program, special consideration in granting loans must be given to students who plan to teach.

To evaluate requests for such student loans, the Office of Education set up on 9 July a consultant panel of college administrators. This panel will study the requests for funds made by colleges and universities. Under the NDEA, loans are not granted directly by the government. Rather, funds are allotted to educational institutions, which then make the actual loans to qualified students.

Implementing another provision of the Defense Education Act, the Office of Education has recently issued the first federal loan to strengthen the science-teaching program of private nonprofit schools. The Tennessee Military Institute, the first of 186 schools to receive such a loan, borrowed $7500 to purchase

equipment for science teaching. To date, applications for loans from schools in this category total $2,431,000. Private schools which are operated for profit are not eligible for loans under the act.

### Exotic Language Programs

Under other provisions of the act, the Office of Education has completed arrangements to open 19 centers for instruction in languages rarely taught in this country. Persian, Singhalese, and Swahili are among the languages to be offered next fall at some of the new centers. The instruction offered at the various centers will not be limited to languages. Courses on economic conditions and cultures of the countries or areas where these languages are spoken will also be offered.

The centers will be located at various universities throughout the country and operated by the universities. Some of the centers will offer instruction in only one language; the center at the University of Kansas, for example, will offer only Chinese. The two centers at Harvard will offer seven languages—Chinese, Korean, Tibetan, Arabic, Persian, Turkish, and modern Hebrew. The center at the University of Chicago will offer five. Other languages offered by one or more of the 19 centers for the coming academic year include Russian, Hindustani, Japanese, Portuguese, Bengali, Burmese, Finnish, Hungarian, Polish, Turkish, Vietnamese, and Indonesian. Among the most rarely taught languages are Gujerati and Marathi (spoken in South Asia), Yoruba (spoken in Africa), 25 Uralic-Altaic languages (spoken throughout Asia and Eastern Europe), Khalkha (spoken in Outer Mongolia), Thai, and Telugu (spoken in India).

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## Computers and Automatic Stations To Speed Weather Forecasting

Under a contract recently awarded, work will start on the development of a new weather forecasting system that will employ computers and automatic observation stations. The new system, which is being sponsored by the Air Force, the Federal Aviation Agency, and the Weather Bureau, is expected to reduce by two-thirds the interval between observation and forecast. Such a reduction, if achieved, would result in much more accurate predictions of rapidly changing weather conditions.

The saving in time will be accomplished primarily by the substitution of automatic devices for human observers, telegraph operators, and forecasters. Data on pressure, temperature, humidity, and wind direction, which are now collected largely by human observers, will be gathered by automatic weather stations. These data will be transmitted by telegraph printers operating at 600 words a minute—about ten times the speed of the human operators in the present network. The system will be controlled by a large electronic computer, which will signal each station when to report. The computer, replacing the human forecaster, will process the data and transmit its prediction to automatic display devices in airport control towers, pilot briefing rooms, and local Weather Bureau offices.

This system, which will be virtually automatic, is expected to reduce the interval between observation and forecast to 2 hours or less. With the present network, this interval is sometimes as long as 6 hours. It is expected that use of the automatic system will eventually reduce the interval to 20 minutes.

### Test Network Set for 1963

A test network, employing the new system, is scheduled to go into operation in the northeastern United States by 1963. Ten stations, eight of them major civilian or military airports, will be linked together in the test net. The stations are: L. G. Hanscom Field at Bedford, Mass.; Westover Air Force Base at Chicopee, Mass.; McGuire Air Force Base at Wrightstown, N.J.; New York International Airport (Idlewild); Suffolk County Air Force Base at Westhampton, N.Y.; the Atlantic City Weather Bureau; Washington National Airport; Oceana Naval Air Station at Norfolk, Va.; Teterboro (N.J.) Airport; and the National Meteorological Center in Suitland, Md.

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## Salt Mine Sought for Nonnuclear Detection Shot

The Atomic Energy Commission is negotiating with the Carey Salt Company for use of the company's mine near Winnfield, La., for detonation of several nonnuclear, high-explosive charges. The purpose of the firings is to check the seismic records of such explosions. It is expected that there will be about ten detonations of charges ranging up to 5 tons in size.

As was pointed out in the summary of the "Findings of the Panel on Seismic Improvement," which was released on 12 June, it is possible that seismic signals from underground explosions could be reduced through decoupling techniques by a factor of 10 or more. (Theoretically, the signals might be reduced by a much greater factor than this.) In view of its importance to the current Geneva discussions, it is hoped that the results of the high-explosive tests in the Carey salt mine will be of help in evaluating this theory.

Instruments will be placed at various locations within the mine and on the surface relatively close to the mine. The U.S. Coast and Geodetic Survey will assist the Atomic Energy Commission in placing and operating the instruments, and in analyzing the signals received. It is expected that the firings will commence late this summer. Preparation for, and conduct of, the work will be the responsibility of the test manager of the Atomic Energy Commission's Albuquerque Operations Office; the project will be under the technical direction of the Lawrence Radiation Laboratory, operated for the Atomic Energy Commission by the University of California.

## AAAS To Hold International Oceanographic Congress

The AAAS will hold another special meeting this year, the first International Oceanographic Congress, according to Dael Wolfle, executive officer. UNESCO and the Special Committee on Oceanic Research of the International Council of Scientific Unions are cosponsoring the meeting, which will be held at the United Nations headquarters in New York from 30 August through 11 September. Participants are expected from all parts of the world. Oceanographic vessels from France, the U.S.S.R., and the United States will be in New York harbor and may be visited by those attending the congress.

The AAAS committee, consisting of Mary Sears (chairman), Gustaf Arrhenius, John Cushing, Fritz Koczy, Gordon G. Lill, George S. Myers, Roger Revelle, Henry M. Stommel, and Lionel A. Walford, has been at work for over a year on the arrangements and has planned an excellent and very full program. This will be the first full-fledged oceanographic congress ever to be held and will bring together both biological and physical scientists interested in prob-

lems of oceanographic research. The congress will open with a reception on the evening of 30 August, and the first scientific session will be held the morning of 31 August. The meeting is open to anyone who wishes to attend. The registration fee of $10 covers program materials, a book of abstracts of the technical papers, and admission to several of the social events.

A preliminary announcement of the AAAS International Oceanographic Congress was published in *Science* 27 March. Further details on the congress and final revisions of the program will be printed in News of Science in the issue of 14 August.

## Forest Reserve Program

Since 1947 the Society of American Foresters has been endeavoring to locate and permanently protect representative units of virgin or old-growth forest for purposes of future research in silviculture, forest management, and related ecological fields. Such forest units, called "natural areas," must be at least 20 acres in size; no upper size limit has been set, but for purposes of the program areas of more than 1000 acres would not be needed. A single type of forest growth within each area must cover at least 10 acres. Timber-cutting and grazing are prohibited on natural areas, and use of these forests for recreational purposes by the general public is discouraged.

The Society of American Foresters has recognized 156 forest-cover types for the United States as a whole, 106 in the East and 50 in the West. To date the society has been instrumental in protecting 49 cover types in the East and 36 in the West. Fifty-four percent of the 156 cover types are presently found within the approved natural areas. However, only 46 percent of the eastern cover types are protected, and the percentage is still lower for the southeastern region.

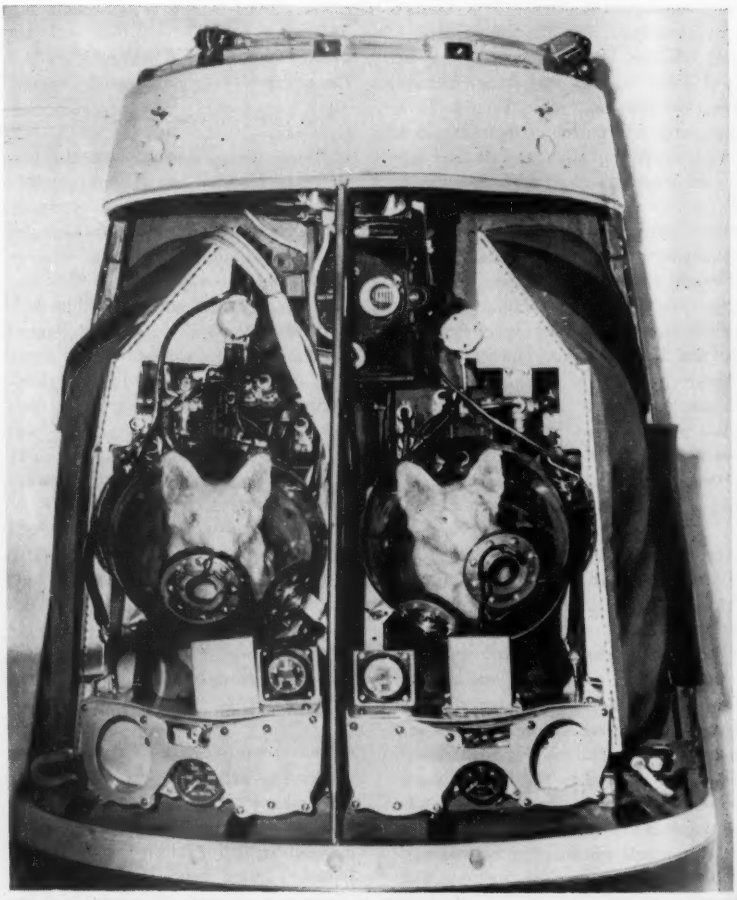
To assist the society in this program, Resources for the Future, Inc., has granted funds which will make possible a field investigation in the states of North Carolina, South Carolina, Georgia, Florida, Alabama, Mississippi, and Tennessee. The society will appreciate the assistance of anyone who knows of forests in those states which warrant consideration as natural areas. Anyone having such information should communicate with John F. Shanklin, Chairman, Committee on Natural Areas, Society of American Foresters, Mills Building, 17th and Pennsylvania Ave., NW, Washington 6, D.C. If possible, the information should include: (i) the names of both the state and county, with specific data on actual location within the county; (ii) the area (in acres) of each type of forest growth; and (iii) the name and address of the owner.

## Rabbit and Two Dogs Recovered from Space

The Soviet Union sent two dogs and a rabbit into space on 2 July and successfully recovered them. The payload of more than 4400 pounds (this includes the weight of the animals) was said to be the heaviest yet put into space. No altitude for the successful shot was given in the Soviet announcement.

Tass, the official Soviet press agency, said that the instruments aboard the missile had sent back information on the animals' reaction to weightlessness as well as information on the ultraviolet part of the solar spectrum, the structure of the ionosphere, and the direction and speed of air streams at various altitudes.

The condition of the animals was reported to have been good on their return. One of the dogs, whose name means "courageous" or "daring," had been used in two earlier flights, according to the announcement. When the launching and recovery were revealed, 4 days after the event, Soviet commentators cited the significance of the flight for eventual human travel in space. "This has proved we can bring animals back alive," one commentator said. "It means much in the preparation for space flights by human beings." None of the radio and press announcements mentioned the successful launching and recovery in this country of the monkeys Able and Baker.



Models were used in this Soviet rocket chamber designed to carry a pair of dogs aloft for short space flights. The chamber, enclosed in a parachute pack, was exhibited at an industrial achievement fair in Moscow last February. A similar chamber was presumably used 2 July when two dogs and a rabbit were sent into space and successfully recovered.

## Darwin Centennial Celebration

An international Darwin Centennial Celebration marking the centenary of the publication of the *Origin of Species* will be held at the University of Chicago in November 1959. The celebration will open 24 November, the centennial day, and continue through 28 November. It is being sponsored by the University of Chicago with the cooperation of national scientific societies and with financial aid from the National Science Foundation and the Wenner-Gren Foundation for Anthropological Research. Meetings will be held on the university campus.

The celebration will bring together leading scientists to discuss the evolution of life, of man, and of the mind. Centering on problems that cut across disciplinary lines, the discussions will comprise five 3-hour panels open to the public. These will concern the origin and nature of life, the evolution of life, man as a biological organism, the origin and nature of mind, and social and cultural evolution. Each panel will trace the growth of evolutionary knowledge over the past 100 years, forecast trends of research and thought in the next 100 years, and appraise the significance of these developments for mankind. The discussions will be presented by 47 specialists in biological and cultural evolution. The papers and the discussions will be published later.

All members of the AAAS are invited to attend the celebration. Since the response to early invitations was greater than had been expected, the number of those who want to attend may exceed the number of accommodations available. Those wishing further information should write at once to: Conference Director, Darwin Centennial Celebration, University of Chicago, 1126 E. 59th St., Chicago 37, Ill.

## Waste Disposal Report

A panel of the National Academy of Sciences–National Research Council has reported that "certain Atlantic and Gulf of Mexico coastal areas can be used as receiving waters for the controlled disposal of packaged, low-level radioactive wastes." Low-level wastes are composed, in the main, of the trash from industrial and academic research laboratories, hospitals, and research institutions that have been licensed by the U.S. Atomic Energy Commission to use small quantities of radioactive materials for scientific research or in the diagnosis and treatment of illness.

In its report, the panel recognized the necessity for spotting such disposal areas at many points along the Atlantic and Gulf coasts in order to provide for the use of low-level radioactive materials at a greater number of locations. After intensive study of local oceanographic conditions, the panel was able to recommend 28 possible disposal sites and to specify the amount of various radioactive isotopes that could be safely dumped at these sites in suitable containers. The 28 recommended sites included every major seaport area—from Boston, Mass., to Corpus Christi, Tex.—where offshore currents and other conditions made waste disposal safe.

The report of the panel, "Radioactive Waste Disposal into Atlantic and Gulf Coastal Waters," is now available as publication No. 655 in a series of scientific and technical reports of the National Academy of Sciences–National Research Council and may be purchased from the printing and publishing office of the organization at a cost of $1.

## Reactor for Teaching

Columbia University's new subcritical atomic reactor has been designed with great operating flexibility so that it will be a versatile teaching aid for training nuclear engineers. Designed by Edward F. Leonard, director of graduate laboratory work for Columbia's new nuclear science and engineering program, the graphite pile is shielded by an array of reversible paraffin slabs that permit rapid change-over of the exponential pile from reflected to unreflected operation.

The pile consists of 270 bars of high-purity, nuclear-grade graphite. Each bar is 4 inches square and 60 inches long. A square notch was cut from one corner of each of 113 of the bars to allow for insertion into the pile of aluminum tubes containing uranium-bearing fuel slugs. Neutrons are fed into the pile from the bottom layer of graphite bars, where a plutonium-beryllium neutron source can be placed in various locations.

Funds for fully implementing Columbia's new nuclear science and engineering program were included in a grant from the Atomic Energy Commission as part of its continuing program to encourage the training of nuclear engineers in the face of an acute shortage of qualified technical personnel.

## News Briefs

An agricultural observation team departed 8 July for the Soviet Union under the U.S. Government's scientific and technical exchange agreement with the U.S.S.R. The group is the seventh agricultural team to go to the Soviet Union since the agreement became effective last year, and the first to leave this summer. Its purpose will be to gather technical information regarding biological control of pests. The U.S.-Soviet agreement provides for the visit of a total of nine U.S. agricultural technical groups to the Soviet Union in 1958 and 1959, and for a similar number of teams from Russia to visit the United States in the same period. Six Soviet teams came to this country last summer, and, in exchange, six U.S. groups went to Russia.

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Information on the world's nuclear research reactors, now in use or planned for engineering studies and other atomic investigations, has been published by the United Nations in volume 10 of the 33-volume *Proceedings* of the Second U.N. International Conference on the Peaceful Uses of Atomic Energy. The volume on *Research Reactors,* containing 548 pages and 769 illustrations, presents the text of 48 papers submitted to the conference (held in Geneva in September 1958) and a record of the oral discussions at sessions on research and engineering reactors.

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Abstracts of a symposium on "Extramural Science Program of the Federal Government," held at the December 1958 annual meeting of the AAAS, may be obtained from the Washington Academy of Sciences, 1530 P St., NW, Washington 5, D.C.

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A meeting was held on 12 July in the Jacques Loeb Laboratory of the Hopkins Marine Station, Stanford University, in commemoration of the centenary of the birth of the famous general physiologist Jacques Loeb. Speakers included his son, Leonard Loeb of the University of California, A. R. Moore (one of Loeb's students), and L. R. Blinks, director of the Hopkins Marine Station. Letters from Loeb's brother, Leo Loeb of Washington University, and from W. J. V. Osterhout, J. H. Northrop, Hardolph Wasteneys, Moses Kunitz, and other friends and colleagues were read. A collection of Loeb's books and reprints was on exhibit.

## Scientists in the News

Ten University of California physicists led by Nobel laureate EDWIN M. McMILLAN, director of the Lawrence Radiation Laboratory, are visiting Russia this summer to attend scientific conferences. McMillan and eight other physicists attended the 9th International Conference on High Energy Physics, held at Kiev from 20 to 25 July. The other Berkeley scientists in the group were LUIS ALVAREZ, GEOFFREY CHEW, ROBERT KARPLUS, BURTON MOYER, and EMILIO SEGRE, professors of physics; M. L. STEVENSON, assistant professor of physics; EDWARD J. LOFGREN, physicist in charge of the Bevatron; and BRUCE CORK, physicist in the Lawrence Radiation Laboratory. WILLIAM B. FRETTER, professor of physics, attended the meeting in Moscow of the International Union of Pure and Applied Physics.

J. I. HARRIS, member of the Medical Research Council's external staff in Cambridge, England, will attend a course on bacteriophage at Cold Spring Harbor, New York, in August.

ARMIN H. FREI, physicist with the Institute of Advanced Electrical Engineering, Zurich, Switzerland, has joined the staff of the Electronic Research Laboratory at the Radio Corporation of America's David Sarnoff Research Center, Princeton, N.J.

J. P. FULTON, professor of plant pathology at the University of Arkansas, has been appointed head of the department of plant pathology. He succeeds E. M. CRALLEY, who has been named director of the university's Agricultural Experiment Station.

ALVIN F. GARDNER, research fellow at the National Institute of Dental Research, National Institutes of Health, Bethesda, Md., has been appointed associate professor of pathology at the University of Maryland.

EUGENE COPELAND, former executive secretary of the division of research grants of the National Institutes of Health, Bethesda, Md., has been appointed professor and chairman of the department of zoology at Tulane University's College of Arts and Sciences. He succeeds FRED R. CAGLE, who recently became university coordinator of research.

JULES FREUND, head of basic studies on allergy and immunology at the National Institutes of Health, Bethesda, Md., has received a special citation from the Allergy Foundation of America for his contributions to medical science.

CARL F. KOSSACK, former head of the department of mathematics and statistics at Purdue University, has joined the research organization of the International Business Machines Corporation at the Lamb Estate Research Center, Town of Cortlandt, N.Y., as manager of the newly formed statistics and operations research department.

BRAGE GOLDING, director of research for the Lilly Varnish Company, Indianapolis, Ind., has been named head of the new School of Chemical Engineering at Purdue University. Purdue's School of Chemical and Metallurgical Engineering has recently been divided into two separate schools. REINHARDT SCHUHMANN, JR., formerly head of metallurgical engineering, became head of the new School of Metallurgical Engineering. EDWARD H. COMINGS, who headed the combined school for several years, has resigned and has been appointed dean of engineering at the University of Delaware.

CALVIN A. PAGE, professor and head of the department of bacteriology at Southwestern Louisiana Institute, has been appointed director of research at the Microbiological Research and Consulting Laboratory, Orlando, Fla., effective 1 September.

WILLARD A. KREHL, associate professor of biochemistry at Yale University, has been appointed associate professor of medicine and an associate in biochemistry at Marquette University School of Medicine, Milwaukee, Wis.

CURTIS P. CLAUSEN, chairman of the department of biological control, Agricultural Experiment Station, University of California, Riverside, recently retired and became professor of biological control emeritus.

DAVID E. DAVIS, associate professor of comparative behavior and assistant dean in the School of Hygiene and Public Health at Johns Hopkins University, has been appointed professor of zoology at Pennsylvania State University, effective 1 September.

ALEXANDER SANDOW, professor of biology at New York University, has been appointed member in charge of the Division of Physiology at the Institute for Muscle Disease, Inc., New York, N.Y. The institute is a new research establishment sponsored by the Muscular Dystrophy Associations of America, Inc. Sandow will continue at New York University as adjunct professor of biology in the Graduate School.

## Recent Deaths

Rev. AGOSTINO GEMELLI, Milan, Italy; 81; psychologist and philosopher; founder and president of the Roman Catholic University of the Sacred Heart and president of the Pontifical Academy of Sciences; 15 July.

GERARD E. GRAF, South Bend, Ind.; 28; assistant professor of biology at the University of Notre Dame; 20 June.

MURRY HACKMAN, Paramus, N.J.; 39; weather research scientist at New York University; 12 July.

WALTER HAUSER, Sharon, Conn.; 66; retired research curator of Near Eastern archeology at the Metropolitan Museum of Art, New York; 13 July.

PERRY S. HORENSTEIN, Bellport, N.Y.; 54; surgeon and a founder of the Brookhaven Memorial Hospital in Patchogue, N.Y.; 13 July.

CHARLES H. MINER, Wilkes-Barre, Pa.; 91; former state secretary of health of Pennsylvania; a pioneer in the care of tuberculosis patients; 12 July.

HENRY A. PERKINS, West Hartford, Conn.; 85; former head of the physics department of Trinity College; president of the American School for the Deaf for 42 years; 15 July.

GILBERT MORGAN SMITH, Palo Alto, Calif.; retired professor of botany at Stanford University; former vice president of the AAAS, president of the Botanical Society of America, and president of the American Microscopical Society; 11 July.

GEORGE A. STETSON, Stamford, Conn.; 70; editor of *Mechanical Engineering*, official publication of the American Society of Mechanical Engineers from 1930 until his retirement in 1956; associate professor of heat power engineering at New York University, 1924–28; assistant professor of mechanical engineering at the Sheffield Scientific School of Yale University, 1920–23; former president of the Yale Engineering Association; 20 June.

# Book Reviews

**The World of Insects.** Paul Pesson. Translated by R. B. Freeman. McGraw-Hill, New York, 1959. 204 pp. Illus. $15.

**Grassblade Jungle.** Nesta Pain. Coward-McCann, New York, 1959. 207 pp. Illus. $3.75.

**Collecting, Preserving, and Studying Insects.** Howard Oldroyd. Macmillan, New York, 1958. 327 pp. $5.

These three books have little in common, except that in one way or another they deal with insects and related arthropods.

It is a rare pleasure to pick up a handsomely done "picture book" and discover that it has an informative, technically accurate text. This pleasure awaits one who opens Pesson's excellent volume, *The World of Insects*. The reader will find in it a rather detailed account of the geologic history, evolution, physiology, ecology, behavior, and economic importance of insects, as well as a brief summary of their classification. Included are discussions of such technical subjects as hormonal control of growth and differentiation, the function of various types of compound eyes, mechanisms for water conservation, communication in social insects, mating behavior, and resistance to insecticides. The treatment is generally lucid, but, although in most cases the necessary terms are sufficiently well defined, a glossary would have been a valuable addition. As in any book of this scope, some errors have crept into the text (for example, adult crane flies are described as predators). I also take exception to a few of the statements on evolution.

Ninety-six plates (16 in color) add to the appeal of the book. Unfortunately the figures do much less to complement the text than one would expect in view of their abundance. The impression given is that the illustrations were assembled independently of the text and were coordinated with the text as an afterthought. With a few exceptions the figures are excellent. The color plates are overly intense but, in general, pleasing. Of special interest are three plates showing, respectively, the same species of grasshopper against six different backgrounds, a trio of protectively colored caterpillars, and a striking photograph of a vespid wasp drinking from a drop of water.

*Grassblade Jungle* is a well-written account of the biology and behavior of honey bees, mantids, termites, cicadas, scorpions, and grasshoppers, based on the writings of entomologists such as Fabre and von Frisch. The book is primarily intended for the layman, but the professional biologist could well spend a pleasant evening reading it. The author manages quite well to avoid the commonest pitfalls of this sort of writing —anthropomorphism, romanticism, and gross inaccuracy. The volume would have benefited greatly from the inclusion of some carefully selected photographs of the animals described.

One would expect Oldroyd's 327-page book, *Collecting, Preserving, and Studying Insects*, to be a valuable aid in a field of rapidly improving techniques such as modern entomology. Unfortunately it is not. Some of the techniques described are obsolete or obsolescent. A number of modern methods of handling specimens are glossed over or ignored. The reader interested in methods of collecting and preserving insects might consult the fine little manual by Ross (available free from the Illinois Natural History Survey) or the more comprehensive book by Beirne *et al.* (Canada Department of Agriculture, Publication No. 932).

Oldroyd's book has an unusually poor set of illustrations, and some are inane. We are given a full-page halftone of a net bag folded over the hoop "so that nothing can escape"; two halftones of a boy with his head in a net ("best way to corner a captive insect"); and numerous inferior line drawings of bottles, boxes, forceps, and the like.

The "studying" part of the work is devoted to a none-too-detailed set of instructions for the most unimaginative, antiquated type of taxonomic work. No attention is paid to the more dynamic aspects of entomology; the behavior, ecology, evolution, and physiology of insects are ignored. Even the dissection of soft tissues "is a subject too big to be dealt with." (Two pages of concisely written text could give enough information on tools and techniques to be of tremendous value to the beginning student.)

There is useful information scattered through the book, but the items not covered as well or better in less expensive sources are so few and far between that this book can only be recommended to the most affluent.

PAUL R. EHRLICH
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**Landslides in Clays.** Alexandre Collin, 1846. Translation by W. R. Schriever and others. Forward by R. F. Legget. Memoir on Alexandre Collin by A. W. Skempton. University of Toronto Press, Toronto, Canada, 1956. 160 pp. Illus. + plates. $6.50.

This book presents the major work of Alexandre Collin (1808–1890), whose name is almost lost to civil engineering, and reveals that Collin was a forerunner of the present-day practitioners of soil mechanics. Those interested in the origins and development of the principles and practices of civil engineering are indebted to Schriever, Legget, Skempton, and their assistants for the dedicated and painstaking work that went into the translation and preparation of this volume. In spite of the difficulties of translating the original French, the text is clear and readable. The memoir by Skempton summarizes Collin's contribution to modern soil mechanics and presents a brief biography.

Collin was the first to measure the shear strength of clay samples and to survey accurately and analyze mathematically the form of rotational slips in clay strata. His work exemplifies the coordination of field experience, field observation and measurement, laboratory testing and research, and theoretical analysis of the total body of data to a degree not frequently seen today. In 12 chapters Collin summarized his observations, laboratory experiments on the strength and volume stability of various types of clay with varying water

content, and the results of his theoretical analysis of the causes and engineering control of slides in clays.

Collin's methods and conclusions opened a new era in civil engineering. He recognized the significance of geologic relationships in engineering practice and differentiated clearly between cohesive and cohesionless soils. He concluded that the slip surface is the result, not the cause, of failure, as had been previously proposed by his contemporaries. The fundamental cause of failure of clay slopes was determined to be inadequate shear strength. He showed that such features as the type of clay and the conditions of service to which the slope is exposed may determine the stability. He described and examined theoretically various procedures for the prevention and repair of slides on slopes and embankments.

The book includes 21 plates which are photographic reproductions of Collin's drawings of the profiles of slides described in the text, his geometrical methods of analysis, and methods of preventing and repairing slides. The value of the plates is restricted by the fact that most of the symbols to which Collin refers in the text are illegible.

RICHARD C. MIELENZ
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**Our Atmosphere.** Theo Loebsack. Translated from the German by E. L. Rewald and D. Rewald. Pantheon, New York, 1959. 256 pp. Illus. $5.

The rediscovery of the romance of the atmosphere has brought forth an outpouring of popular books in all languages. Not since the heyday of the free balloon has the public imagination been so caught by the phenomena of the earth's air mantle. With space travel only a step away, many of the physical and chemical problems of the atmosphere, which seemed utterly academic only a decade back, are now practical questions. And the ever-changeable weather, with its influence on human activities, has been with man since the dawn of his existence and will remain a governing environmental factor for a long time to come.

As a guide into this realm, the biologist and science writer Theo Loebsack gives us a well-rounded picture. His exposition grips the reader's imagination as he takes us through the world between the earth's surface and space. The origin of the atmosphere, its composition, the sound phenomena, and the optical properties, from blue sky to rainbows and mirages, are well sketched. Polar lights and the endless procession of clouds are adequately described. The nature of winds and weather phenomena, including the drama of hurricanes, tornadoes, and jet streams, comprises a substantial part of the book. Professional meteorologists will appreciate the author's plea for tolerance of their efforts at forecasting. Loebsack also tries valiantly to dispel many myths about cycles, as well as myths that originated in the "hundred-year calendar"—a European forerunner of some of our weather almanacs.

A slight difficulty for the American reader is introduced by the translators' use of British terminology throughout. More serious, however, are the large number of small inaccuracies and loose statements. Here is a random sample of these. One reads that colors of the sky can be caused by comparatively low humidity. The physical laws of the atmosphere are credited to Leverrier. There is a badly scrambled diagram of tornado occurrences and the basic flow patterns that lead to tornadoes. The conflagration of Hamburg, one is told, added water vapor to the air to cause rainfall. The buran is made into an African snowstorm. The word *tension* is misused, in one place to designate potential and in another to indicate tonus. The role of blizzards in destroying crops of coffee, corn, and bananas in the Gulf of Mexico area is considerably overstated. It should have been easy to avoid these slight errors by proper review, and one is surprised to find credit given in the preface to certain experts for such an effort.

It is a pity to have to note these lapses in an otherwise well-written book, which has some very unusual and appealing features. Among these are the interesting discussions on the possible role of an earlier stage of the atmosphere in the generation of life, and the chapter on the relation of the air environment to breathing. The appraisal of radioactive fallout and radiation damage by a neutral observer is sobering.

The book is well illustrated and contains some excellent color reproductions, but the proofreading was carelessly done. On the whole, the merits of the book far outweigh the faults; these could be easily eliminated in another printing.

H. E. LANDSBERG
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**Traité de Biochimie Générale.** vol. 1, parts 1 and 2, *Composition Chimique des Organismes.* P. Boulanger and J. Polonovski. Masson, Paris, 1959. 1476 pp. Illus. Cloth, F. 24,500; paper, F. 22,000.

This large handbook of biochemistry will be published in three volumes. Volume 1 deals with the chemical composition of organisms; volume 2, with the enzymes; and volume 3, with the metabolic processes and their coordination. The first of these volumes has been published, in two parts. The editors emphasize in their foreword that the work is written to acquaint students with the present state of biochemistry and to supply them with references to sources where they will find directions for experimental work. Two of the principal initiators of this work, M. Polonovski and M. Javillier, died before publication of the first volume. The present editors are M. Florkin, P. Boulanger, M. Lemoigne, J. Roche, and R. Wurmser.

In the first chapter of part 1, M. Javillier and D. Bertrand discuss the discovery of different elements in organisms and their concentration in organs and tissues. The same authors treat the inorganic compounds of biological importance in a second short chapter. The bulk of the two volumes is formed by chapter 3, which deals with the organic body constituents. In a last short chapter F. Tayeau discusses complexes which are formed by the permanent or temporary combination of two or more of the simpler materials. Lipoproteins, glycoproteins (mucoproteins), glycolipids, and complexes formed by antigen plus antibody, by apoenzyme plus coenzyme, or by enzyme plus substrate are briefly treated in this chapter, which, as the author points out, forms a link between classical biochemistry and biophysics.

Chapter 3, which comprises 1254 out of a total of 1397 pages of text, consists of a short introduction, written by Roche, and six other sections. In the first of these Lespagnol discusses the reactions, derivatives, and syntheses of carbohydrates. The very logical classification and nomenclature seem to be typical of French biochemistry; the designation *heterosides* is used for complexes formed by the conjugation of sugars with aglycons. In the next section Naudet and J. Polonovski report on fatty acids, glycerides, phospholipids, and "cerides," while sterols are treated by Bar. The term *cerides* is used for waxes, glycerol ethers, and similar derivatives. A subsequent short section is devoted to the discussion

of isoprene derivatives, phenols, and organic acids. The last three sections deal with proteins and their derivatives. These sections are introduced by an excellent discussion on the general chemistry of amino acids (P. Desnuelle), followed by a similar section on peptides, written by P. Boulanger and G. Bizerte. The same authors also describe the isolation, the properties, and the classification of the proteins; the physical chemistry is treated by Wurmser and Tonnelat. In the section "Heteroproteins" J. Montreuil reports on glycoproteins, on phosphoproteins, and, in collaboration with P. Boulanger, on nucleoproteins; chromoproteins, discussed by Roche, Bar, and Bizerte, are classified as porphyrinic and nonporphyrinic pigments. In the final section of the chapter on proteins various amines and alkaloids are discussed by Nguyen van Thoai and Robin.

I was impressed by the logical, clear-cut classification and presentation of most of the material. This makes the work extremely valuable to students and other persons who want to know more about biochemistry than a good textbook can offer. Most of the chapters contain a wealth of references, so the reader can easily find more specific literature.

The question may be raised as to whether in the present era of rapid progress in biochemistry the publication of such a voluminous handbook is justified. An enormous amount of time and effort on the part of outstanding biochemists is consumed in such a project. I feel that the time has passed when handbooks can be written by experts of a single nation, and that the writing of handbooks of this type sooner or later will be a cooperative enterprise to be undertaken by scientists of many nations. Despite my opinion on this point, however, I consider the *Traité de Biochimie Générale* an admirable survey of the present state of biochemistry and believe it has great value as a didactic and reference work.

FELIX HAUROWITZ
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**Ecological Processes.** Alan Mozley. Lewis, London, 1959. xi + 68 pp. 9s.

By "ecological processes," Mozley means groups of changes occurring with respect to time in ecological situations in nature. He illustrates these processes chiefly from studies of fresh-water mollusks harmful to man—his own field of research. Little is said about other organisms in the ecosystems discussed.

The most interesting sections of the book are those dealing with animals in changing environments. Succession in animal communities has not been thoroughly studied. Although no quantitative data are presented or referred to, the author's suggestion that pest species are most successful in intermediate stages of a sere, following disturbances of a habitat, is of both scientific and practical importance.

A number of statements, such as "Personally, I expect the numbers of animals in a given place to fluctuate more or less," detract from the book. Much of the material is not expressed in a quantitative manner, nor are there many references to research reports of original data. This serves to underscore the need for quantitative observations and adequately designed experiments in this field, as the author indicates. However, in some sections—for example, those on fluctuations and population stability—important advances are not mentioned.

In my opinion, population ecologists will not find this slender volume especially stimulating. The book seems intended rather to advance the author's plausible suggestion, based on extensive observations, for controlling intermediate hosts of parasites and other pest animals.

ALAN J. KOHN
*Division of Zoology,*
*Florida State University*

**Notes of a Soviet Doctor.** G. S. Pondoev. Consultants Bureau, New York; Chapman and Hall, London, 1959. iii + 238 pp. $4.95.

This is an awkward translation of a cloying, chauvinistic "doctor book" written by an elderly Russian physician "for young doctors . . . and medical students." It reads more like a lecture on the facts of life delivered to a group of none-too-bright adolescents by an elderly deacon, not too bright either, but bright enough to stay out of trouble.

A chapter purported to describe the origins of medicine mentions Harvey, ties Darwin and Marx into one sentence, and from there on stays within the Russian borders. It will be interesting to see whether Chinese histories of medicine will contain as many Russian names!

To Western ears, the proud assertions that "the Soviet doctor is a government agent" and that this gives him freedom and assures "the highest moral standard," sounds like double talk. In our society, the individual and government are competitive, and balance is maintained through a dynamic equilibrium of checks and diversity. In Soviet society, the individual and the government are one, and competition is unthinkable.

In these times, when species suicide is a real possibility instead of a mad dream, an understanding of the differences between the two cultures must be attempted. Reading this book in this context may be of some small assistance, but the road is rough. If only the Soviets could develop a touch of a sense of humor and see the world in colors and shades instead of in the dreary black-and-whites of the Marxist-Leninist religion!

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**Circumpolar Arctic Flora.** Nicholas Polunin. Oxford University Press, New York, 1959. xxviii + 514 pp. Illus. $20.20.

Although the circumpolar arctic flora is a small one in number of species, any attempt to describe it is fraught with discouraging difficulties. The exact location of the southern boundary of "the arctic" is a matter of controversy, so more or less arbitrary limits have to be set in the listing of species. Vast areas in the north are still unexplored or poorly explored from the botanical standpoint. Although there has been a resurgence of arctic botanical investigation during the past three decades, international communication among botanists has not kept pace with it. Last but not least, a relatively large proportion of the species are polymorphic and difficult to define.

Polunin has recognized and dealt with all of these difficulties in the introduction to the present volume. The introduction also includes a map which defines "the arctic" for immediate purposes, an explanation of how the book is to be used, and a statement about the truly impressive source materials upon which it is based. The treatment is confined to the vascular flora and opens with a key to the families. Keys to the genera appear in the ensuing text, and if there are more than eight species in a genus, descriptive keys to these species are provided. All the descriptions of

genera and species are comprehensive and excellent. They are supplemented with hundreds of line drawings, most of them superb. Polunin will, inevitably, be criticized for the way he has treated a few of the species, but he has been wise in remaining fundamentally conservative in his treatment of most of them. The frequent appearance of the abbreviations "agg." and "s.l." after specific names leaves both the author and the user of the book maximum latitude for interpretation.

One could wish that more references to the literature on difficult polymorphic species had been added to this volume instead of being reserved for another. Nevertheless, the book will be essential to anyone working with the arctic flora. For the first time in botanical history we have a work that constitutes a forthright attempt, by a qualified student, to describe all the basic species of vascular plants known to occur in the arctic.

HUGH M. RAUP
*Harvard Forest, Harvard University*

**Fundamentals of Ornithology.** Josselyn Van Tyne and Andrew J. Berger. Wiley, New York; Chapman and Hall, London, 1959. xi + 624 pp. Illus. $11.75.

This book, begun by the late Josselyn Van Tyne and completed by Andrew J. Berger, is intended as a textbook of ornithology at the college or university undergraduate level. It is a serious, scholarly work and a meticulously written book, above the average usually found in a textbook, and shoud serve its purpose very well indeed. Because of their accuracy and maturely reasoned presentation, the various chapters form digests and summaries of the special topics they treat that should be useful, as convenient references, to more advanced readers as well. The detailed descriptions of all the 160 or so recognized bird families of the world are the best and most readily assimilable short summaries available, and, as such, it seems not at all unlikely that they may well become standard, easy references for professional workers in the field.

Anyone who has attempted to organize the data and then to write reasonably short, and yet selectively inclusive, accounts of fields that are very actively expanding and changing—fields that are suddenly becoming open to new modes of study and to new expository approaches, such as the analysis of behavior patterns and of the impact of new devices such as radar on the factors affecting and directing migratory movements of birds—will appreciate the difficulties encountered by the authors and will be impressed by the success of their efforts. A good textbook, as compared with a merely adequate one, must not only give the student, or the nonstudent reader, the basic information on the subject but must also present it in such a way as to stimulate his thinking about it and whet his appetite. To a degree quite unusual in college texts, this volume succeeds in doing this. In my considered opinion, the Van Tyne-Berger book should meet with wide acceptance and should have a decidedly useful and progressively beneficial and important influence in training potential students of avian biology. To the biologist working in fields or specialties other than ornithology, the book may be not only a handy compendium of existing information but also something of an eye-opener as to the amount of integrated knowledge that exists concerning the most completely surveyed of all comparable animal groups.

The book is not only well conceived and well written; it is also well illustrated with numerous pen-and-ink drawings, by George Miksch Sutton, of a great variety of types of birds. These illustrations are uniformly excellent and add much to the appearance of the volume, illustrating and decorating it at the same time.

HERBERT FRIEDMANN
*U.S. National Museum,*
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**Mineralogy.** An introduction to the study of minerals and crystals. Edward H. Kraus, Walter F. Hunt, Lewis S. Ramsdell. McGraw-Hill, New York, ed. 5, 1959. ix + 686 pp. Illus. $9.

The main features that have characterized earlier editions of this popular text, of which the first appeared in 1920, are retained in this new edition. The numerous figures are mainly halftone reproductions of photographs. They include many portraits of well-known mineralogists and crystallographers and photographs of wooden models of crystals, which the authors seem to prefer to the more widely used line drawings. In the chapters on morphological crystallography (chapters 2 to 9, 88 pages) the Weiss parameters are still given the same prominence as Miller indices, being listed in every table and in the legend of every figure in which symbols for crystal forms are given. Though there is a brief explanation of axial elements and of how they may be determined from measured interfacial angles, only a single example of axial elements appears in the chapters dealing with particular crystal systems, and none in the descriptive section.

Many minor changes and small rearrangements have been made throughout the text. Among the more important changes is the addition, in the descriptive section, of Hermann-Mauguin symbols to characterize crystal classes. The chapter on chemical mineralogy, retitled "Chemical mineralogy and crystal chemistry," has been expanded by the addition of three and a half pages on crystal chemistry. The material in chapter 14, now entitled "Formation and occurrence of rocks and minerals," has been rearranged and expanded to give more emphasis to what Landes has called "geological mineralogy." In the descriptive section there has been some rearrangement in the order of treatment of silicate minerals, presumably with a view to taking account of structural relationships, but the chondrodite group has been separated from the olivine group, with which it is most closely related structurally, and placed between topaz and datolite. The formulas for chrysotile, members of the epidote group, and a few other minerals have been changed to correspond with the results of recent work.

The concluding chapters, "Gemstones" and "Classification of minerals according to elements," represent the special interests of some of the members of the department of mineralogy at the University of Michigan. The title of the latter chapter is a bit misleading, for this chapter (of over 40 pages) deals at length with the uses of minerals and their products and is illustrated by photographs of power shovels, stock piles, open pits, ore carriers, and industrial plants.

The glossary and the tabular classification of the 32 crystal classes (unfortunately separated from the chapters on crystallography by nearly 400 pages) are unchanged, as are the tables for the determination of minerals, which occupy nearly 170 pages. Several new items have been added to the selected bibliography.

A. PABST
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## New Books

*Advances in Psychiatry.* Recent developments in interpersonal relations. Mabel Blake Cohen, Ed. Norton, New York, 1959. 314 pp. $4.95. The articles in this book are selections reprinted from *Psychiatry.* All the selections were published within the last 20 years.

*The Coconut Palm.* A monograph. K. P. V. Menon and K. M. Pandalai. Indian Central Coconut Committee, Ernakulam, S. India, 1957. 400 pp. $11.95.

*Control Engineering.* Gordon J. Murphy. Van Nostrand, New York, 1959. 397 pp. $7.50.

*The Earth and Its Resources.* Vernor C. Finch, Glenn T. Trewartha, M. H. Shearer. McGraw-Hill, New York, ed. 3, 1959. 592 pp. $6.

*Elementary Practical Hydraulics of Flow in Pipes.* A text and reference book for engineers and draughtsmen. C. T. B. Donkin. Oxford Univ. Press, New York, 1959. 221 pp. $3.40.

*Elements of Calculus and Analytic Geometry.* George B. Thomas, Jr. Addison-Wesley, Reading, Mass., 1959. 590 pp. $7.50.

*Faune de France.* vol. 62, pt. 3, *Coléoptères Curculionides.* Adolphe Hoffmann. Lechevalier, Paris, 1958. 632 pp.

*Fundamentals of Freshman Mathematics.* C. B. Allendoerfer and C. O. Oakley. McGraw-Hill, New York, 1959. 488 pp. $6.50.

*Fundamentals of Physical Science.* Konrad Bates Krauskopf. McGraw-Hill, New York, ed. 4, 1959. 663 pp. $6.95.

*General Chemistry.* A systematics approach. Harry H. Sisler, Calvin A. Vanderwerf, Arthur W. Davidson. MacMillan, New York, ed. 2, 1959. 861 pp. $7.95.

*The Great Pulse.* Japanese midwifery and obstetrics through the ages. Mary W. Standlee. Charles E. Tuttle, Rutland, Vt., 1959. 192 pp. $4.50.

*Hypersonic Flow Theory.* Wallace D. Hayes and Ronald F. Probstein. Academic Press, New York, 1959. 478 pp. $11.50.

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# Reports

## Effect of the Solar Wind on the Lunar Atmosphere

*Abstract*. The extent of the lunar atmosphere is severely limited by collision with the protons of the solar wind.

The composition and extent of the lunar atmosphere have been the subject of considerable speculation by physicists and astronomers (*1–5*). We have carried out calculations which indicate that the lunar atmosphere will be limited by collisions with protons in the solar wind, if the flux of particles in this wind has the value given by Biermann (*6*). We find, in fact, that for a typical constituent, the limiting density of the lunar atmosphere is lower by a factor of $10^{11}$ than would be estimated in calculations neglecting the solar wind.

The properties of the lunar atmosphere are usually assumed to be governed by two competing processes: (i) the accumulation of gases from the surface of the moon, and (ii) the loss from the atmosphere of a certain fraction of those molecules whose thermal energies are sufficient to permit their escape from the gravitational attraction of the moon.

If we assume an isothermal atmosphere and a Maxwellian distribution of particle velocities, with an infinite mean free path, then a calculation based on the balance between these two processes leads to the following result for the equilibrium number density of atmospheric molecules at the lunar surface (*7*):

$$n_0 = J\sqrt{\frac{\pi m}{2kT}}\left(1+\frac{mMG}{kTR}\right)^{-1}\cdot\exp\left(\frac{mMG}{kTR}\right) \quad (1)$$

In Eq. 1, $J$ is the number of molecules ejected from the lunar surface per square centimeter per second. $T$ is the lunar atmospheric temperature; $m$, the mass of an atmospheric particle; $M$, the mass of the moon; and $R$, its radius.

Probable sources for the lunar atmosphere are: (i) argon produced by the radioactive decay of potassium, and (ii) the production of such gases as $SO_2$, $CO_2$, and $H_2O$ by residual volcanic activity. Estimates of the emission of these gases based on terrestrial data suggest $J \simeq 5\times10^5/\text{cm}^2$ sec for argon, and $J \simeq 10^{10}/\text{cm}^2$ sec for volcanic gases (*5*).

The rate of escape from the atmosphere is determined by the maximum temperature, for which thermocouple measurements indicate a value of 370°K (*8*). Substituting these values of $J$ and $T$ into Eq. 1, we find $n_0 \simeq 5\times10^{15}/\text{cm}^3$ or $10^{-4}$ atm of argon. A similar calculation for the volcanic gases leads to pressures greater than 1 atm for $SO_2$ and $CO_2$, and to $4\times10^{-8}$ atm for $H_2O$. However, the latter values will be substantially reduced when allowance is made for the effects of photodissociation and chemical reactions with the crust, and for the fact that the age of the moon ($\simeq 4.5\times10^9$ yr) does not allow enough time for $SO_2$ and $CO_2$ to build up to equilibrium (*5*).

In addition to the above gases, we also expect traces of xenon and krypton. Using the ejection rates for these gases as calculated by Edwards and Borst (*9*) in Eq. 1, we find their contribution to the lunar atmosphere to be negligible, compared to that of the above gases.

The above calculations neglect the solar wind. The solar wind is usually assumed to consist of protons with a number density of $10^3/\text{cm}^3$ and velocity of $10^8$ cm/sec (*6*). In an elastic collision with an atom these protons will transfer an average of 1 kev of kinetic energy, which is sufficient for the escape of the atom. If we assume that every atom struck by a proton escapes, the rate of ejection of particles from the atmosphere is (per cubic centimeter),

$$R_p = n_a n_p V_p \sigma_{el} \quad (2)$$

In Eq. 2, $n_p$ is the number density of protons, $V_p$ is their velocity, and $\sigma_{el}$ is the proton-particle elastic cross section. We may now estimate the equilibrium particle density at the lunar surface by equating the rate of ejection of particles from a vertical column 1 cm² in cross-sectional area, as calculated by Eq. 2, to the rate of injection of particles, $J$, into the column at the surface. This gives:

$$n_0 = \frac{J}{n_p V_p \sigma_{el} h} \quad (3)$$

In Eq. 3, $h$ is the scale height of the isothermal atmosphere ($kTR^2/mMG$).

Taking $\sigma_{el}$ to be $10^{-16}$ cm², we find $n_0 \simeq 10^4/\text{cm}^3$ or $10^{-15}$ atm for argon. For the volcanic gases we find $n_0 \simeq 10^8/\text{cm}^3$ or $10^{-11}$ atm. This value represents an upper limit for these gases because photodissociation has been neglected.

Comparing these values with the results obtained above, we see that the solar wind reduced the density of the lunar atmosphere by a factor of $10^{11}$ for argon, $10^4$ for $H_2O$, $10^{15}$ for $CO_2$, and $10^{22}$ for $SO_2$.

Our remarks depend on the assumption that the moon's magnetic field is too feeble to shield the atmosphere effectively from the solar wind. The magnetic field of the moon has been estimated to be less than 200 γ's (*10*). A comparison of the magnetic pressure of such a field with the pressure of the solar wind indicates that a field of this intensity at the surface of the moon would have practically no shielding effect for the atmosphere.

Finally, we note that the estimates of the lunar atmosphere based on the limiting effect of the solar wind are consistent with the experimental upper bound of $10^{-8}$ atm established by coronograph measurements (*11*). The respective values for argon and the volcanic gases bracket the estimate of $10^{-13}$ atm, which has been reported by Ellsmore and Whitfield (*2*), on the basis of measurements of the lunar occultation of radio sources (*12*).

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12. We thank R. Jastrow and I. Harris for several discussions during the course of this investigation.

22 June 1959

*Instructions for preparing reports.* Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

## Generalization from Pain of Various Types and Diverse Origins

*Abstract.* Pain that arises from very different pathological origins responds in a quantitative fashion to a given dose of a given narcotic, but this is not true when the pain has been contrived by experimental means, in man, with customary techniques.

Among the problems in the study of pain is the question of whether observations made on one kind of pain from one source are comparable to observations made on another kind of pain from another source. There are two great sources of pain, experimental and pathological. If generalization is possible, one should be able to show that study of pain of various types from widely differing origins leads to the same results. An admirable opportunity to make such an examination is presented in the work of two groups of investigators—the Harvard group who for years have studied the acute pain arising from surgical wounds and the Sloan-Kettering group who have studied, also for years, the chronic pain of malignant disease. It is possible to find a significant common factor for study in the two groups: it is the relationship between a given dose of morphine or other analgesic agent and the pain relief produced by it, expressed in quantitative terms.

Specific data from the two groups are given in Table 1.

If this level of excellent agreement had occurred once only, one might have supposed that this was merely happy coincidence, even though extraordinary. The fact is, such agreements between these two laboratories (and among others as well) have, over the years, been common, notwithstanding the fact that the two groups have studied pain of very different origins. A recent example of agreement, arrived at through a somewhat different approach from that described above, is the following, a comparison of the work of Seed, Wallenstein, Houde, and Bellville (*1*), on dihydrocodeine with the work of Gravenstein, Smith, Sphire, Isaacs, and Beecher (*2*).

Seed *et al.* took the difference between the pain relief effected by each dose of dihydrocodeine and that effected by the standard 10-mg dose of morphine as a measure of analgesic effect. Thus, one can calculate a straight-line log dose-effect curve, using the method of least squares. They did this, with suitable weighing of the effect at each dose in accordance with the number of patients involved, and the following equation was obtained

$$Y = 50.32 \log X - 89.32$$

where $Y$ is percentage difference between the effects of the given doses and $X$ is the dose of dihydrocodeine in milligrams. When $Y$ is zero, $X$ is 59.6 mg. Since the Harvard group's maximum dose was 45 mg this calculation represents an extension of their data.

The Sloan-Kettering group approached the problem in a second way. This time they studied the effects of the different doses of morphine versus the effect of the standard 10-mg dose of morphine in the data of Keats, Beecher, and Mosteller (*3*) and derived the following equation,

$$Y = 57.55 \log X - 58.99$$

Using this equation and the similar one for dihydrocodeine, they calculated equivalent analgesic doses. They found 18.76 percent less pain relief for 5 mg of morphine than for 10 mg. From the equation for dihydrocodeine it was calculated that 25.2 mg of dihydrocodeine would produce the same degree of analgesia as 5 mg of morphine, and for 10 mg of morphine the equivalent dose of dihydrocodeine would be 50.4 mg. The Harvard group's finding that 59.6 to 50.4 mg of dihydrocodeine (the value depends on the method of calculation employed) produces the same degree of analgesia as 10 mg of morphine is in remarkable agreement with the value of 53 mg obtained by the Sloan-Kettering group when they studied peak effects during the first or second hour, as the Harvard group had done.

But in the case of experimental pain the situation is different. The current lack of such reproducibility of result in experimentally produced pain in man (animals are another matter) is striking. Some 15 groups of investigators (see *4*) have failed to demonstrate that the experimental pain threshold in man varies dependably with even large doses of morphine or other analgesic agents, whereas small doses of morphine will relieve the pain of a great wound or extensive disease.

Clearly, there is an important difference between the two types of pain, experimental and pathological, in terms of response to analgesia. There is some evidence at hand about what this difference is: true anxiety or fear appears to be missing in experimental pain. When anxiety or fear has been injected into the experimental pain situation, evidence has been obtained that experimental pain then responds to morphine as pathological pain does [see Malmo and Shagass (*5*) and Hill *et al.* (*6*)].

Other factors may be found to account for the difficulties encountered in the use of experimental pain in man to appraise analgesic agents (experimental pain is very useful in studies with animals). In our own unpublished work with experimental pain produced by tourniquet, where pain intensity grows slowly (as contrasted with the sudden stab of pain produced by most experimental methods), we have found promising leads to suggest that experimental pain slowly produced *may* be useful in appraising analgesic agents. (There may of course be an anxiety factor here; the matter needs further study.) Houde (*7*) has suggested that in most studies of experimental pain possibly too much reliance has been placed on mechanical contrivances, gages, and so on, and that perhaps it would be better to rely more completely on patients' statements. This is, of course, what is done in appraising pathological pain.

One must conclude that there is a difference between the two types of pain in man, as generally produced to date. Whether this difference is qualitative or quantitative (one suspects that it is the latter) is beside the immediate point. There is reason to believe that pain of both experimental and pathological origin consists of two components—(i) the original sensation and (ii) the psychic reaction or processing component—and that the second is dominant in pathological pain, whereas the first is dominant in experimental pain.

There is such uniformity of response to analgesic agents of pathological pains of widely differing origin in man that one can utilize this response to quantify analgesic agents, as Keats, Beecher, and Mosteller (*3*) showed in comparing one series of "unknown" morphine solutions with another series of unknown morphine solutions. While this demonstration in one laboratory is of interest, interest is greatly broadened by the demonstration, discussed here, of quantitative reproducibility in two laboratories dealing with pathological pain of very different origins.

These findings lead to the following conclusions. (i) Pain arising from widely different pathological sources responds in a remarkably precise, quantitative fashion to a given dose of a given narcotic. (ii) No such demonstration has yet been made in man for experimental pain as commonly produced; why this is so requires further study. (iii) Cautious

Table 1. Pain relief effected in man by parenteral injection of 10 mg of morphine and by a placebo. The studies of Lasagna and Beecher (*8*) were of postoperative wound pain; the study of Houde and Wallenstein (*9*) was of chronic pain from cancer.

| Study | No. of patients | Percent relieved by | |
|---|---|---|---|
| | | Morphine | Placebo |
| *Lasagna and Beecher* | | | |
| 1952 | 66 | 65.8 | 39.0* |
| 1953 | 56 | 69.3 | |
| *Houde and Wallenstein* | | | |
| 1952–53 | 57 | 65.0 | 42.0 |

* Averaged placebo data from Lasagna, Mosteller, von Felsinger, and Beecher (*10*).

generalization concerning pathological pain from study of specific pathological pain is permissible. (iv) No such generalization from experimental pain in man to pathological pain in man is as yet permissible. There is some acceptable evidence that the response is comparable when a powerful anxiety component has been introduced into the experimental pain situation. How broad the limits of usefulness of this finding may be is yet to be shown. Other, as yet unknown, factors may be pertinent.

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16 February 1959

## Maximal Photosynthetic Rates in Nature

*Abstract.* It seems likely that turbulence under natural conditions, both aquatic and terrestrial, is higher than it is in the bottles or leaf chambers used when photosynthesis is measured experimentally. Most of the maximal photosynthetic rates reported in the literature are probably lower than those which occur in nature.

Most previous estimates of photosynthetic yield in aquatic habitats have been based on experiments involving enclosure of phytoplankton communities in clear and dark bottles and exposure of these communities to light of various intensities by suspension at different depths in a lake. The data so obtained are considered representative of the photosynthetic activity of the phytoplankton under natural conditions, and these data are inserted in equations containing factors for light penetration, day length, and phytoplankton abundance (*1*) to provide estimates of yield per unit of water surface. The average hourly yield obtained when the exposure period was long (8 hours or more) was lower than that obtained in exposures of a few hours' duration (*2*), and yields obtained when bottles were agitated exceeded those in quiet bottles (*3*). Doty (*3*) considered the quiet bottle a closer approach to natural conditions than the agitated bottles, and most of the studies in the literature are based on data from quiet bottles. It seems likely, however, that confinement of a phytoplankton community in bottles represents a significant departure from the natural condition and may have a considerable influence on photosynthetic rate. During the summer of 1958, a study (*4*) was carried on in western Lake Erie in which photosynthesis was measured under completely natural conditions and the observed rates were compared with data from parallel experiments in which clear and dark bottles were utilized.

The clear- and dark-bottle experiments were conducted as described in previous papers (*1*, *2*). Phytoplankton communities at natural densities were confined in clear and dark bottles, suspended at various depths (0 to 3 m) at 0.5-m intervals, and exposed for 3 hours. The difference in *p*H (*4*) in the clear and dark bottles was used to determine $CO_2$ absorption by reference to a differential titration curve for the natural water (*2*).

The measurements made under completely natural conditions were made by sampling at approximately 4-hour intervals. Samples were taken from five depths (0.1, 1, 3, 5, and 8 m) at a station in the channel between South Bass and Middle Bass islands where the water was 9 m deep. The samples were returned to the laboratory for *p*H determination, and the change in *p*H during a given time interval was used to compute the change in $CO_2$ concentration. Table 1 shows an example of the data obtained and of computed values for the $CO_2$ change for the water column.

In this study no corrections were applied for $CO_2$ exchange between air and water. The surface water usually was slightly supersaturated in the morning and undersaturated in the afternoon. Bohr (*6*) has shown that when water is stirred vigorously the rate constant for $CO_2$ transport across the gas-liquid boundary layers is about $2 \times 10^{-3}$ cm/sec. Thus, the maximal rate of $CO_2$ entry would be of the order of 9 mmole/m² per 12 hours (*7*) if free $CO_2$ concentration in water were zero. But the actual free $CO_2$ concentration is not zero, and the water is not stirred so vigorously as in Bohr's experiments. The $CO_2$ equilibrium between air and water was usually reached during the morning hours (the average time of this occurrence was 0800), so the net diurnal $CO_2$ exchange must have been in the direction of $CO_2$ absorption from the atmosphere, and neglecting this process makes our estimates of photosynthesis too low. It seems likely that the error is less than 2 percent.

The computations in Table 2 also assume that the *p*H changes due to the influx of different water between sampling times will not introduce a systematic error. Random errors will, of course, be introduced by this factor, so the rate computed from a single day's work may be too large or too small, depending on whether the change of water masses increased or decreased the *p*H value. In the average from several days' work, however, such random errors will cancel out. Values for individual days ranged from 0 to 620 mmole/m² day.

Students of photosynthesis have usually considered two quantities, (i) total or gross photosynthesis and (ii) apparent photosynthesis (that is, photosynthesis in excess of the respiratory processes of the plants which carry on the photosynthetic activity). When one is measuring $CO_2$ change in a natural aquatic environment, a third quantity must be considered—namely, net photosynthesis (that is, photosynthesis in excess of the respiratory contributions of the entire aquatic community). Computations like those in Table 1 represent diurnal net photosynthesis. Because the nocturnal $CO_2$ production is practically equal to the diurnal $CO_2$ uptake (the *p*H value at 0630, for example, shows no consistent trend from day to day), we can obtain an approximate estimate of the gross

Table 1. Carbon dioxide change as computed from *p*H change under completely natural conditions.

| Depth (m) | *p*H | | $\Delta CO_2$ (mmole/m³) | Cubic meters* represented in water column | $\Delta CO_2$ for entire water column (mmole/m³) |
|---|---|---|---|---|---|
| | Time | | | | |
| | 0600 | 1100 | | | |
| 0.1 | 8.24 | 8.42 | 24 | 1 | 24 |
| 1.0 | 8.23 | 8.40 | 24 | 1 | 24 |
| 3.0 | 8.21 | 8.35 | 19 | 2 | 38 |
| 5.0 | 8.20 | 8.31 | 15 | 2 | 30 |
| 8.0 | 8.19 | 8.28 | 12 | 3 | 36 |
| Total | | | | | 152 |

* The first two samples are considered representative of the first two meters; the 3- and 5-m samples, of the next 2-m intervals, respectively; the 8-m sample, of the last three meters in the water column.

Table 2. Photosynthetic yields under completely natural conditions compared with yields computed from light- and dark-bottle data (see *13*).

| Time interval | Rate of photosynthesis (mm/m²/day)* | | |
|---|---|---|---|
| | Under natural conditions | | Computed from bottle data |
| | Net | Gross | Gross |
| 17 June–18 July | 225 | 450 | 187 |
| 19 July–22 August | 288 | 576 | 242 |

* The difference between 225 and 288 is not statistically significant with the number of samples used (14 in each period), but the differences between 450 and 187, 576, and 242 are significant beyond the 1 percent level.

photosynthetic rate by doubling the observed diurnal net rate, for the respiration rate indicated by the nocturnal $CO_2$ gain must continue throughout the daylight hours.

Table 2 shows the average daily $CO_2$ uptake, net and gross, computed in this way and compared with daily gross yields estimated from light- and dark-bottle data. Note that the diurnal net yields exceed the gross yields computed from the bottle data. It is evident that enclosing the aquatic community in bottles reduces photosynthetic activity to less than half that maintained under natural conditions (*8*). One factor which may contribute importantly to the reduced rates in bottles is the greatly reduced turbulence. Eddy diffusivities in western Lake Erie in summer are of the order of 25 cm²/sec; this must be at least $10^4$ times greater than the values for unagitated bottles.

Photosynthetic rates for terrestrial plant leaves have usually been measured by determining $CO_2$ absorption from an air stream passing through a leaf chamber. The highest rates of air flow routinely used have been about 2.5 lit. of air per square centimeter of leaf per hour (*9*). Heinicke and Hoffman (*9*) state that they chose a flow rate of 2.5 lit./cm² hr because they observed that a hydroxide surface when exposed to such a flow rate absorbed $CO_2$ at the same rate as did a hydroxide surface standing open in the room. Such a flow rate, therefore, represents conditions of air turbulence much quieter than those prevailing outdoors.

Heinicke and Hoffman observed an increase in photosynthetic rate (in apple) from 22.1 to 30.8 mg of $CO_2$ per square decimeter per hour when they increased the rate of air flow from 2.5 to 7.5 lit./cm² hr, although the $CO_2$ content of air at this flow rate did not fall below 80 percent of that in normal air. More recently Burnside (*10*) observed that increasing the flow rate from 1 to 6 lit./cm² hr increased the photosynthetic rate (in cotton, and sunflower) from 20 to 37 mg/dm² hr. Most of the maximum yields in Rabinowitch's (*11*) Table 28.VI are less than 20. The rates reported by Russian workers represent notable exceptions. Kursanov's rate for *Pyrus malus* is 35, closely similar to the rate obtained by Heinicke and Hoffman with their high flow rate. Perhaps the higher yields reported by Russian workers reflect higher turbulence in their leaf chambers. A relation of photosynthetic rates to eddy diffusivity in leaf chambers might contribute to our understanding of the problem. Verduin (*12*) has published some computations of probable $CO_2$ concentrations within leaf chambers, but we have found no quantitative study of eddy diffusivity in leaf chambers.

It seems likely that most of the maximal terrestrial photosynthetic rates appearing in the literature are considerably lower than the maximal rates attained under completely natural conditions; the reduced turbulence of the medium in the experimental apparatus may be primarily responsible for the reduced rates.

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7. This computation is made as follows:

$$\underset{\text{(cm/sec)}}{2 \times 10^{-8}} \quad \underset{\text{Concn. difference across boundary layers}}{\times 13 \times 10^{-6}} \quad \underset{\text{Partition coeff.}}{\times 0.8} \quad \times 10^4 \ \text{(cm}^2\text{/m}^2\text{)} \quad \times 4.3 \times 10^4 \ \text{sec/12 hr}$$

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13. A sample computation from bottle data is as follows:

$$\underset{\text{(mm/m}^2\text{/hr) Optimal light}}{4.8} \quad \underset{\text{Ratio of optimal value to av. in euphotic zone}}{\times 0.65} \quad \underset{\text{(m) Depth of euphotic zone}}{\times 5} \quad \times 12 \ \underset{\text{Hours of daylight}}{}$$

26 February 1959

## Magnitude of Reinforcement and Consummatory Behavior

*Abstract.* The rates at which white rats licked saline, sucrose, and saccharin solutions, respectively, were measured by cumulative recording of tongue contacts with the solution in question. The local rate of licking was constant for all solutions, but differences in the distribution of sustained periods of licking were related to the type and concentration of the fluid consumed.

In place of Hull's (*1*) original associative interpretation, Spence (*2*) has recently argued for a motivational theory of the effects on behavior of different reward magnitudes. Spence bases part of his argument upon empirical data collected in his own and other laboratories. But greater attention is given to indirect evidence derived from a theoretical analysis of runway behavior. According to this theory, part of an animal's consummatory behavior in a goal box becomes conditioned to stimuli in that goal box, and ultimately, through generalization, anticipatory goal responses ($r_g$) are elicited by stimuli in the runway. When $s_g$ represents feedback stimulation from the response $r_g$, the mechanism $r_g$-$s_g$ is said to maintain running in the alley. In particular, this $r_g$-$s_g$ mechanism is assumed to possess motivational properties "that vary with the magnitude or vigor with which it occurs" (*2*, p. 135).

Whether or not this assumption is necessary is not certain (see *3*), but it is inadequate as stated, owing to the ambiguity of the term *vigor*. The vigor of a response might, for instance, refer to the magnitude, or strength, of the response—that is, the amount of effort that goes into a single occurrence of that response; it might refer to the rate at which that response occurs (when it does occur); or it might refer to the persistence of that response in competition with other responses. Thus, *vigor* might refer to the intension, tempo, or perseveration of a response, or to any combination of these and other response dimensions (*4*). Only the first and second of these dimensions appear to have been considered by Spence—that is, he questions whether different magnitudes of reward produce different $r_g$'s in terms of strength or intension (*3*, p. 137) or whether the effect is on the tempo of a single $r_g$ (*2*, p. 147), and, with some reservations, he concludes that the second alternative is correct—on the basis of experiments reported by Guttman (*5*) and by Sheffield, Roby, and Campbell (*6*). However, neither of these studies is pertinent, because although they show that different reward conditions lead to different operant and consumption rates, there is no indication whether these were due to the tempo, perseveration, or latency of the consummatory behavior. That is, the consummatory response may have oc-

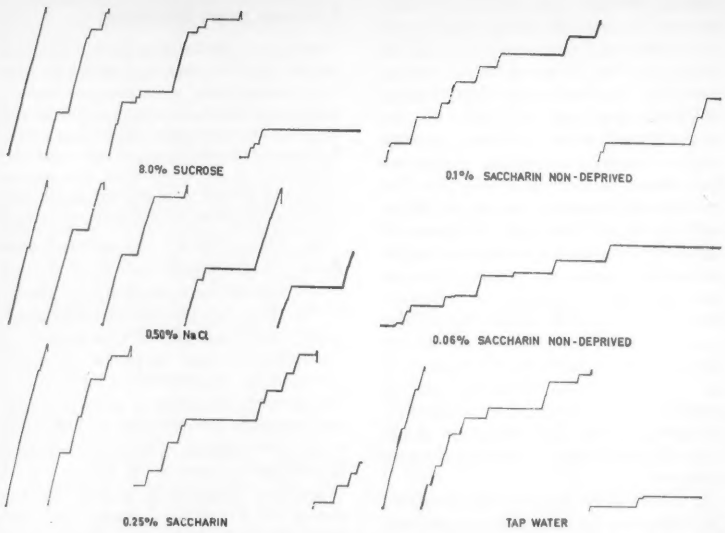


Fig. 1. Cumulative curves of the response of one animal given access to the six fluids indicated. Unless otherwise indicated the animal had been deprived of water for 23½ hours prior to the test. Each full excursion of the pen represents 1000 licks, and each graph represents 30 minutes of drinking. Thus, for example, when the deprived animal was offered 0.50-percent NaCl, it made approximately 4500 tongue laps in a 30-minute period.

curred more slowly (when it did occur) with smaller rewards rather than larger ones; it may have occurred at the same tempo but less often in the sense that more, or longer, pauses intervened between sequences of consummatory responses; or it may have begun at different latencies after the reinforcing solutions became available.

Despite Spence's contention to the contrary (*2*, p. 144), it is possible to measure consummatory behavior directly in a Skinner-box situation. This method, first employed by Kappauf and later by Stellar and Hill (*7*), has been utilized by one of us to record the drinking behavior of white rats when presented with various concentrations of sucrose, saccharin, and sodium chloride in distilled water solutions.

The apparatus was a 15- by 6- by 6-in. compartment wired so that a small current (less than 1 μa) flowed each time the animal's tongue made contact with the fluid under test. This current was amplified sufficiently to operate a relay which activated a Gerbrands-type cumulative recorder. Each lick of the fluid from a drinking tube with a 3-mm opening, which protruded into the cage, advanced the pen one step across the recording paper, with the result that a curve directly reflecting the rate of consumption of the fluid in the tube was automatically produced. The apparatus was installed in a large darkened and sound-shielded room. None of the noises of the apparatus were audible to the human ear from inside the room.

Each one of four rats of the Sprague-Dawley strain was tested with all of the solutions used in the experiment. The animals were first adapted for 10 days to a 23½-hour drinking rhythm, with access to tap water for ½ hour out of each 24 hours. The sodium chloride tests were then run for ½-hour sessions, with 2-day intervals between the test sessions. For the 2 days following each test day the animals were given only tap water to drink, so as to equalize as far as possible the degree of water deficit prior to the tests. If, for example, on a test day a large quantity of the proffered fluid was consumed, the tap-water intake on the following day was reduced. It has been observed, however, that by the third day following a large intake the amount of tap water consumed is about the same as that normally consumed by a rat that has been deprived of water for 23½ hours. The concentrations of the sodium chloride solutions employed were 0.50, 0.86, 2.0, and 3.0 percent, respectively. All the solutions used were percentage (weight/volume) preparations in distilled water. At the end of the sodium chloride tests the same procedure was followed with saccharine and sucrose solutions, in that order. After the licking rates for these solutions for water-deprived rats had been determined, the saccharin series was repeated when the animals had been sated with food and water.

The results in all cases were clear-cut; the local rate of drinking an acceptable solution was constant for each animal. If an animal drank all, it drank at a constant rate, regardless of its state of deprivation and the concentration of the solution under test. Figure 1 presents typical examples of all the records collected and shows some of the licking curves for one of the animals given access to tap water, 0.50-percent sodium chloride, 0.25-percent saccharin, and 8.0-percent sucrose, respectively, when water-deprived and to 0.6-percent and 0.10-percent saccharin when satiated. The curves that were produced by the other three animals are identical with respect to the major characeristic performance.

Although Gilbert (*4*) reports individual differences in tempo of bar pressing, no such differences in licking rates were observed in these studies. The local rates of responding remained between five and six licks per second for all animals under all conditions. Variations in the total amount of intake were a function solely of the duration of the pauses and the lengths of the sustained drinking periods. That is, differences in quantities of liquid consumed per unit of time represent not, as Spence (*2*) appears to believe, differences in the tempo of the consummatory response but differences in the perseveration of that response.

It seems clear, then, that the tempo, although not necessarily the intension (*4*), of the consummatory response must be ruled out as a determiner of instrumental performance with different reinforcing stimuli. If some characteristic of the consummatory act itself is to be used to account for these differences, perseveration seems the most likely, in our current state of knowledge (*7*). Although the local rate of licking is a constant, independent of individual subject differences and the chemical composition of the fluids investigated in this study, other dimensions of the licking operant are not. Latency, perseveration, and duration of the operant may all vary with differing test fluids, as Gilbert (*4*) has demonstrated with another operant. An investigation of the dimensions of the consummatory operant would appear to be necessary before an adequate explanation of the effect of the quantity or quality of a reinforcing substance on instrumental behavior can be given. Clearly the amount of reward substance consumed per unit of time is too coarse a measure. It does not describe how the substance is consumed. It tells nothing about the temporal distribution of pauses, about the duration of sustained periods of drinking, or about the rate of drinking when it occurs (*8*).

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6 March 1959


## Control of Oöcyte Development in Cockroaches


*Abstract.* Development of ovarian eggs and secretory activity of colleterial glands are inhibited by the oötheca in the oöthecal chamber of *Blattella germanica* (L.) and by the oötheca in the uterus of *Pycnoscelus surinamensis* (L.). The inhibition is due to nervous stimuli from pressure of the oötheca. Removing the oötheca or severing the ventral nerve cord eliminates inhibition of the *corpora allata* and results in premature development of the oöcytes and resumption of activity of the colleterial glands.


Oöthecal eggs in the uterus of the cockroach *Leucophaea maderae* (Fab.) are said to release a substance which causes the brain to inhibit secretion of the *corpora allata* and, thus, maturation of oöcytes (*1*). Differences in ovipositing behavior among various species of cockroaches (*2*) indicate that this mechanism would operate only in species carrying their oöthecae internally throughout embryogenesis.

*Blattella germanica* carries its oötheca externally during embryogenesis, and the oötheca inhibits development of the oöcytes and secretion of the colleterial glands. Removal of the egg case from the female results in an increase in the rate of oötheca production (*3*). The oötheca is carried an average of 30 days (*4*). During this time the oöcytes increase in length only slightly, from 0.34 to 0.52 mm. Ten days after the first oötheca is dropped the oöcytes have grown to an average length of 2.55 mm, and a second oötheca is formed. The *corpora allata* were shown to be necessary for development of the oöcytes of *B. germanica*; allatectomized adult females did not oviposit. Implantation of *corpora allata* into allatectomized females resulted in oviposition within 2 weeks. An allatectomized female with four implanted *corpora allata* produced three oöthecae in 61 days; another produced four in 58 days. When imitation wax "oöthecae" were inserted into the oöthecal chamber of adult females 1 day old or less, the oöcytes remained undeveloped. When the ventral nerve cord was severed in a female carrying an oötheca, the oöcytes grew rapidly and colleterial glands accumulated secretion in spite of the presence of an attached oötheca [Fig. 1 (1 and 2)]. Nineteen females from which oöthecae were removed 3 to 5 days after oviposition formed new oöthecae in 19.8 ± 0.5 days (*5*). Similarly, 11 females whose nerve cords were cut 3 to 5 days after oviposition formed new oöthecae in 19.5 ± 0.5 days. The original oötheca may adhere to the new one, as shown in Fig. 2. One female with a severed nerve cord formed four oöthecae over a period of 82 days, essentially doubling the rate of oöthecal production.

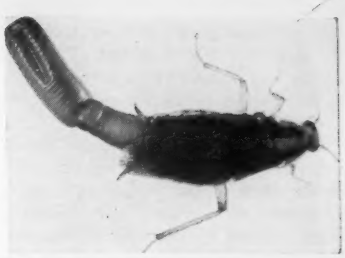

Fig. 2. *Blattella germanica* whose ventral nerve cord was severed 4 days after oviposition; 19 days after the operation it had formed a second oötheca, to which the first adhered. (× 2.4). [E. R. Willis]

*Pycnoscelus surinamensis* incubates its eggs in a uterus, like *Leucophaea maderae* (*2*). During gestation, which averaged 55.5 days in our parthenogenetic strain of *Pycnoscelus* (*4*), the oöcytes increased slightly in length, from 0.56 to 0.75 mm. About 2 weeks after parturition, oviposition again occurs, when the oöcytes average 3.2 mm in length. Usually about 70 days elapse between the formation of the first and second oöthecae. Allatectomized adult females failed to oviposit, but when *corpora allata* were implanted, these females oviposited 2 or more weeks later. The presence of an oötheca in the uterus inhibited ovarian development; the interval between the production of successive oöthecae was decreased from about 70 to 27 days when oöthecae were removed between 1 and 7 days after oviposition. The presence of parts of oöthecae implanted into the body cavity of adult females 1 day old or less failed to inhibit development of the oöcytes. Parts of young oöthecae were implanted into the body cavities of six females 1 day old or less; after 11 days the oöcytes of these six females were 2.91 ± 0.06 mm long—a length similar to that (2.93 ± 0.06 mm) of oöcytes of normal 11-day-old females.

The oötheca was removed from the uterus of each of ten females 1 to 16 days after oviposition, and one half of each oötheca was implanted into the body cavity of the donor female. Twenty-three days after the operation the oöcytes averaged 2.70 ± 0.10 mm in length. However, substitution of a wax oötheca for a real oötheca in the uterus inhibited oöcyte development. Ten females whose nerve cords were cut either 0, < 1, or 4 days after oviposition had well-developed oöcytes 2.74 ± 0.18 mm long and

Fig. 1. (1, 2) Reproductive tracts of *B. germanica* females that carried oöthecae for 26 days. (about × 4.8). (1) Unoperated female; the ovaries (arrows) are undeveloped and the colleterial glands lack secretion; (2) female whose ventral nerve cord was severed 6 days after oviposition; the basal oöcytes are almost mature, and the colleterial glands are full of secretion. (3, 4) Reproductive tracts of *P. surinamensis* females that have oöthecae in their brood sacs. (about × 4.1). (3) Unoperated female 37 days after oviposition; the ovaries (arrows) are small, and the colleterial glands lack secretion; (4) female whose ventral nerve cord was severed just after oviposition; 29 days later the basal oöcytes have matured and the colleterial glands are full of secretion.

full colleterial glands 24 to 31 days after the operation [Fig. 1 (3 and 4)]. When real or wax oöthecae were present in the uterus, oöcytes developed only after the nerve cord was severed.

In both *Blattella germanica* and *Pycnoscelus surinamensis*, inhibition of oöcyte development during pregnancy appears to be due to nervous stimuli resulting from pressure of the oötheca—on the uterus in *P. surinamensis* and on the oöthecal chamber in *B. germanica*. There was no indication that any substance released by the eggs in the oötheca acted through the brain to inhibit secretion of the *corpora allata*, as has been reported for *Leucophaea maderae* (*1*).

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11 March 1959

## Fault Zone along Northern Boundary of Western Snake River Plain, Idaho

*Abstract.* Gravity, seismic, and geologic studies indicate that at least 9000 ft of aggregate throw along a zone of northwest-trending, high-angle faults has displaced the western Snake River Plain downward relative to highlands on the north. At least 5000 ft of movement occurred between the early and middle Pliocene. Progressively diminishing movement since then amounts to 4000 ft.

Movement within a northwest-trending zone of high-angle faults, producing the abrupt escarpment that rises along the northern boundary of the western Snake River Plain in Idaho, was first recognized during reconnaissance surveys 60 years ago (*1*) but was disregarded in subsequent discussions that attributed the Snake River Plain to downwarping (*2*). Recently published gravity and seismic studies support the role of faulting as a cause of the escarpment, and current geologic mapping by the U.S. Geological Survey helps to date the times of movement. The fault zone aligns middle Pliocene and younger rocks of the Snake River Plain against a variety of older rocks in highlands to the north, including granite of the Idaho batholith north of Mayfield and silicic volcanics of early Pliocene age north of Mountain Home.

The pertinent gravity measurements are in a network of 800 locations that embraces southern Idaho and adjacent parts of neighboring states. In this network the greatest change in gravity is across the northern boundary of the Snake River Plain, amounting to a difference of 166 milligals between a high in the Snake River Plain 10 mi southwest of Mountain Home and the low in the Idaho batholith (*3*). This difference in gravity is opposite to that associated with high-angle faults elsewhere in western United States and indicates that rocks in the downthrown block of the Snake River Plain are heavy compared with the Idaho batholith. From analysis of a 50-milligal residual anomaly associated with the steep gravity gradient near Mountain Home, it is calculated that from 13,000 to 38,000 ft of rocks about as dense as Columbia River basalt have been dropped down against the Idaho batholith (*4*), the thickness of the displaced rocks depending on the density contrast assumed (0.3 and 0.1 g/cm$^3$, respectively).

The crustal break implied by the gravity measurements is possibly expressed by a line of earthquake epicenters that extends diagonally from Puget Sound, across the Columbia River Plateau, along the northern boundary of the western Snake River Plain, and thence across the plain to northern Utah (*5*). In Idaho, these earthquakes originate principally at average depths of 61 and 38 km (38 and 24 mi), the shallower earthquakes being near the base of the crust (*6*). The displacement calculated from the gravity measurements therefore ranges from one-tenth to one-third of the local crustal thickness.

Geologic evidence of intermittent crustal adjustment has been found near King Hill along the zone of faulting inferred from the geophysical observations, indicating that rocks of early Pliocene and younger age are dropped downward toward the Snake River Plain in progressively diminishing amounts. Thus, the bounding escarpment is formed by a large displacement of the silicic volcanics of early Pliocene age; basalt of middle Pliocene age that overlaps these faulted volcanics is offset a lesser amount; sedimentary rocks of late Pliocene age overlying the faulted basalt are somewhat offset and disarrayed by faulting; deposits of early Pleistocene age resting on the disturbed late Pliocene rocks are dropped down only a few hundred feet; and, finally, the deposits of middle Pleistocene and younger age are fractured, but not measurably offset, along the fault zone. The detailed paleontologic evidence by which this rock sequence has been dated will be described elsewhere, but the amounts of progressive crustal displacement can be summarized briefly.

The faulted silicic volcanics north of the fault zone are part of an eroded highland as much as 7500 ft above sea level, whereas drilling in the Snake River Plain 15 mi south of the fault zone reached a depth of 1200 ft below sea level without encountering these volcanics (*7*); this seemingly demonstrates a minimum displacement of about 9000 feet. The basalt of middle Pliocene age forms an upland 3800 ft above sea level north of the fault zone, but at King Hill, near the southern margin of the zone, drilling reached a depth of 1600 feet above sea level without penetrating this basalt (*8*); this suggests an offset of about 2200 feet. Displacement of the sedimentary rocks of upper Pliocene age is shown by exposures mainly along the northern margin of the fault zone, where one prominent fault offsets these rocks 600 ft, but tilting, no doubt caused by concealed faults within the fault zone, accounts for an equal amount of additional displacement, aggregating about 1200 ft. The deposits of early Pleistocene age along the fault zone are stepped down by a series of faults whose combined throw is about 600 ft. As the sum of all the measurable displacements in rocks younger than the silicic volcanics (4000 ft) accounts for less than half the minimum total displacement along the fault zone (9000 ft), most of the movement must have occurred before deposition of the middle Pliocene and younger rocks, but after eruption of the early Pliocene silicic volcanics (*9*).

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22 May 1959

## Oceanographic Phenomena in the Arctic Basin

We wish to take exception to an interpretation of oceanographic phenomena in the arctic basin based on measurements for tritium content as presented by Giletti and Kulp [*Science* **129**, 901 (1959)]. On the basis of the agreement between the tritium value of one sample from 400 m ($0.9 \pm 0.1$ tritium unit) and the value for average Atlantic surface water prior to Project Castle ($1.0 \pm 0.1$ TU), Giletti and Kulp have concluded that the sample must be recent, perhaps less than 2 years from the surface. Then, by a series of assumptions, they suggest that this water came from the local continental shelf, and this suggestion is magnified in their conclusions to "(iv) a new source of the deeper water of the Arctic Ocean was found to be in the Canadian archipelago. . . ."

It is generally meaningless to try to interpret vertical relationships of water masses without considering the density structure. In order for water to sink with a minimum of mixing from the surface to depth, whether this sinking occurs more or less vertically as in a convection cell, obliquely as along a tilted isopycnic surface typical of the major oceanic convergences, or following the slope of the bottom in association with cooling and freezing processes as in the Weddell Sea, the water when it leaves the surface must be of a density equal to or greater than the ambient water through which (or under which) it sinks. Ice formed in the freezing of sea water generally contains less salt than the sea water from which it was frozen. Accordingly, as continued freezing occurs, the ambient water is enriched in salt.

For sea water within the commonly occurring range of salinity and temperature, the density at atmospheric pressure, of which sigma-*t* (density at atmospheric pressure $-1) \times 10^3$ is a common measure, can be increased by decreasing the temperature (surface cooling) or by increasing the salinity, which in high latitudes is associated with freezing. Once ice forms, both these surface processes occur simultaneously, with further loss of heat, and this is the case in the Arctic Ocean and Canadian archipelago in the cooling cycle of the year. If compression due to pressure is disregarded, water, once it has left the surface, can increase in density only through mixing with ambient water of the same (second-order effect) or greater density. Although mixing with sublying water of greater density can be effected by wind and tide, or by the momentum of the moving stream, local density flow as apparently envisaged by Giletti and Kulp would result in water formed at the local sea water–ice interface sinking along the bottom and reaching the 400-m level with an equal or somewhat lesser sigma-*t* than the water had when it left the surface.

The density of the water at 400 m [0.35°C, 34.89 $^{0}/_{00}$ (note that salinity is given in parts per mill and not percent, as in the article by Giletti and Kulp; this was presumably a typographical error)] expressed as sigma-*t* is 28.02; that at 10 and 80 m is 25.47 and 26.08, respectively. The salinity of water which would have a sigma-*t* of 28.02 at its freezing point is 34.77 per mill, a value much higher than any ever reliably reported for surface waters in the area under consideration. The values of 31.64 and 32.40 per mill at 10 and 80 m, respectively, as given by the authors, are in agreement with other reported values for nearby parts of the arctic, and this is about the range to be expected at the corresponding depths along the fringe of the archipelago. It may be assumed that in peripheral arctic areas of restricted circulation the salinity of the water may be increased locally as a result of freezing, but such an explanation would hardly account for the great quantity of water of high salinity found widespread in the Arctic Ocean at depths greater than about 200 m.

Further, if the salinity was increased by freezing in the Canadian archipelago and the water then moved down the continental slope without mixing, it would have, at the depths being considered, a temperature of about −1.9°C, corresponding to the freezing point of water of salinity 34.89 per mill, instead of +0.35°C as reported. The average salinity of the water in the upper 100 m in the Canadian archipelago is considerably less than 33 per mill, and the average temperature of this water column is considerably less than 0°C (*1*). Even if enough local freezing should occur to raise the salinity from less than 33 to more than 34.89 per mill, there is no local source of heat in the archipelago waters or on the local Arctic shelf that could raise the temperature of locally sinking waters from the freezing point to +0.35°C.

Giletti and Kulp make special reference to conditions at station 11. The bottom water at this station cannot be distinguished from other water in the area through tritium concentration, as apparently no tritium analyses were made for this particular water. Moreover, there is no indication of the number of temperature measurements used to establish the temperature curve shown in their Fig. 1 (the curves in this figure do not agree with the reference cited). Since salinity values are lacking as well, the possibility that malfunctioning equipment led to an erroneous temperature picture cannot be ruled out. However, let us assume that the value of 0.1°C for the bottom water, as read from the curve given by Giletti and Kulp, is correct, and that this water left the surface at −1.9°C and mixed, in sinking along the archipelago slope, with water of Atlantic origin having a temperature of +0.5°C. The resulting mixture, on reaching the bottom at the indicated 480 m, would consist of about 80 percent Atlantic water and 20 percent locally formed water. The possibility that this or a similar sequence of events occurred cannot be ruled out at this time, but adequate temperature and salinity data would be required to prove it. At best, tritium analyses will afford only supporting information.

The view commonly held is that water such as that which appears at 400 m in the portion of the arctic in question comes from the Atlantic Ocean via the Norwegian Sea, suffering some loss of temperature and some decrease in salinity by subsurface mixing with ambient water en route (*2*). This is in line with what we found from examination of unpublished data from a number of oceanographic stations from the arctic basin, in which we considered not only the temperature, salinity, and density but other variables, such as inorganic phosphate. The water in the upper 50 or so meters enters into convective turnover each winter and is an entirely different water mass from that occurring at depths below 200 or 250 m. Between this surface convective layer and the deeper water is a transition zone in which the density increases rapidly with depth below the surface. The sample taken at 80 m lies in the upper portion of this pycnocline, and from our present limited knowledge of winter convection in the area it would seem that the water from this depth may possibly enter into the vertical turnover in some years but not in others. In other words, it may be one or many years away from surface contact. Additional tritium data from this transition layer might help greatly in clarifying the processes involved. This would require samples from various depths from a network of stations. The one sample from 80 m mentioned in the article, although providing food for speculation, of itself tells nothing of the processes.

We suggest that the agreement between the tritium values for the 400-m sample and for average Atlantic water is not at variance with the accepted view of this circulation. We offer the following two alternatives as to why this value is not lower than $0.9 \pm 0.1$ TU.

1) Giletti and Kulp have assumed that any Atlantic water which sinks to

become deeper basin water circulates cyclonically, requiring a travel time of more than 2 years to reach the sampling location. Actually, the pattern of circulation of the deeper waters has not been definitely established. If the water was formed near Jan Mayen Island (a distinct possibility) and traveled the most direct route to the sampling location in 8 months to 1 year, its rate would be only 2 to 3 mi per day, and it would arrive with a tritium content approximately that of average Atlantic water.

2) The measurements, on which the value of 1.0 TU for the average Atlantic water prior to the spring of 1954 are based, vary from 0.62 to 1.5 ± 0.4 TU (*3*). Let us suppose that the 400-m sample had an original value at the surface of nearer 1.5 than 1.0 TU. Then this sample would have had several years in which to traverse whatever portion of the polar basin lay between and still arrive with a tritium content of 0.9 ± 0.1 TU. In other words, the results of the tritium sampling as reported are too sketchy to serve as the basis of any real conclusions concerning the water circulation of the area.

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30 April 1959

The careful study by Barnes and Coachman of our paper, which attempted to show the potential application of tritium as a natural tracer in arctic problems, is appreciated. They correctly suggest that the heading over the column giving the salinity data should read "per mill" rather than "percent."

We agree that interpretation cannot be carried too far on the basis of such a limited number of measurements; nevertheless, these data are pertinent to the problem discussed and help to limit speculation. Moreover, a sufficient number of tracer data could, alone, lead to solution of this problem, contrary to the statement of Barnes and Coachman that these can only supply "supporting information." The practical difficulties are many, however, and obtaining these reconnaissance samples was a considerable achievement on the part of Crary and Goldstein.

It was not suggested in our paper that the earlier interpretation of most oceanographers—that Atlantic-derived water occupies the Arctic Ocean at depths greater than 200 m,—be discarded. From the tritium data, however, it appeared that another source might contribute to the total influx. To quote from our article (page 903): "In this case the amount of water is small and affects only the bottom 100 m. This sinking water mixes with the normal Atlantic-derived water. . . ." Since Barnes and Coachman agree that it is possible to have a mixture of 80 percent Atlantic water and 20 percent surface water at the station in question, there would appear to be no real disagreement on this point.

The two explanations which Barnes and Coachman suggest from comparison of the tritium concentrations of the 400-m sample in the arctic and of average North Atlantic surface water do not appear to be substantially better than that suggested by us. Our suggestion is that the high tritium content resulted from the sinking of surface water during times of freezing and the mixing of this water of higher tritium content with the Atlantic-derived water.

Their first hypothesis requires the assumption that the water at 400-m depth north of Ellesmere Island comes from the vicinity of Jan Mayen Island by a direct route at the rate of 2 to 3 mi per day. This would indeed account for the tritium content, within the range of experimental error, but such westward movement around the north coast of Greenland and the Canadian archipelago would be in conflict with the pattern of flow proposed by Worthington (*1*). It will be recalled that the area studied by him included the Chuckchi Sea and Beaufort Sea and the area of collection of the sample in question.

For their second hypothesis, based on the data of Giletti, Bazan, and Kulp (*2*) for North Atlantic surface water, they base their arguments on the sample with the largest error. Owing to the small size of this sample, the counting statistics gave a standard deviation of ± 27 percent, which essentially overlaps the mean of all values. This large error must be compared with the standard deviation of ± 10 percent obtained for each of the remaining eight analyses. Of the better measurements, the highest value is 1.34 ± 0.1 TU. If this value is used, the time allowed for the cyclonic gyre of 3500 mi is 7 years. This is based on comparison with the value observed at the station in question (0.9 TU) and implies a mean flow rate of 1.3 mi/day. This rate is probably less than the true rate, which should be inferred from the *average* North Atlantic value; calculation on this basis gives 4.8 mi/day.

Not only is this a rather high rate but it requires a high tritium concentration throughout the Arctic Sea. It would be reasonable to expect this layer of higher tritium content to be at least 300 m thick. The cosmic-ray tritium production required to maintain this level in a steady state would be at least equal to that required for the entire North Atlantic surface water. If this arctic water all came from the Atlantic, the natural tritium production would have to be almost double the presently assigned value. If anything, the production rates calculated from observations on tritium reservoirs are already too high (when compared with calculations of production based on cosmic-ray reaction rates), and this makes the hypothesis of doubtful validity.

Further study of tritium in selected localities could easily support or disprove either of the suggestions by Barnes and Coachman. Far more sampling would be needed to "prove" any of the hypotheses discussed.

It is concluded that although more information is required to settle the matter, the natural tritium tracer may provide the means for doing so. Indeed, it has already focused attention on the areas where the critical data are likely to be obtained.

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20 May 1959

## Absorption of Iron from Iron Chelate by Sunflower Roots

Abstract. Roots of decapitated sunflower plants absorbed iron from the ferric chelate of ethylenediamine di(*o*-hydroxyphenylacetic acid), leaving most of the acid in the nutrient solution. The chelating capacity of the nutrient solution increased as iron was absorbed by the plants. Most of the absorbed iron was found in the plant exudate.

Early investigators (*1*) suggested that synthetic chelating agents delivered iron to roots but were not themselves absorbed. Later research (*2*) led to the view that both components of the metal chelate were absorbed. Some emphasis has been placed on equivalent uptake of metal chelate components by plants (*3*), but later reports (*4*) suggest a nonequivalent uptake. The experiments reported here show that sunflower plants

Table 1. Changes in iron (FeEDDHA), total iron, and chelating capacity* of a nutrient solution containing sunflower plants and the concurrent changes in total iron and chelating capacity of the plant exudate.

| Time of sampling of nutrient and exudate (hr): | Nutrient Solution | | | Exudate | |
|---|---|---|---|---|---|
| | Fe (FeEDDHA) (ppm) | Fe (total) (ppm) | Fe* chelating capacity (ppm) | Fe (total) (ppm) | Fe* chelating capacity (ppm) |
| 0 | 5.4 | 5.5 | 0.043 | | |
| 5 | | 5.4 | 0.177 | 3 | 0.008 |
| 10.5 | 4.5 | 4.5 | 0.227 | 25 | 0.004 |
| 16.5 | | 3.5 | 0.282 | 69 | 0.006 |
| 23.0 | 3.6 | 3.2 | 0.289 | 54 | 0.005 |
| 31.5 | | 3.3 | 0.315 | 24 | 0.003 |
| 42.5 | 3.6 | 3.2 | 0.302 | 6 | 0.012 |

* In terms of $Fe^{55, 59}$ chelated.

absorb iron from the iron chelate but, for the most part, leave the chelating agent in the nutrient solution. The chelating agent was ethylenediamine di(*o*-hydroxyphenylacetic acid), designated EDDHA (*5*). This compound gives a deep red color when combined with ferric iron.

The basic nutrient medium used was an iron-free, modified Steinberg solution (*6*) adjusted to *p*H 6.8. Spectrophotometric determinations show that the nutrient solution, alone, does not absorb at 480 mμ. Neither does nutrient containing EDDHA absorb at this wavelength. However, nutrient solutions containing the ferric chelate of EDDHA (FeEDDHA) show a specific absorbance at 480 mμ and the characteristic red color. The absorbance varies with concentration according to the Beer-Lambert law. Consequently, the loss of iron from a FeEDDHA nutrient solution by root uptake is reflected in decreased absorbance and color, whereas the addition of iron to an iron-depleted nutrient (containing EDDHA) results in increased absorbance and color. This report outlines the method by which the difference in uptake of metal chelate components was determined.

Seeds of sunflower (Greystripe), germinated 3 days on moist muslin, were transferred to basic nutrient solution for 15 days' growth. Basic nutrient solution containing 5.5 mg of iron (as FeEDDHA) per liter was prepared, and a 10-ml sample (hereafter designated "prenutrient") was set aside. Thirty sunflower plants, grouped for exudate collection, were placed in the solution. The stems were cut off and fastened in an inclined position. After the prenutrient sample had been set aside (zero time), a 5-ml sample each of nutrient and exudate was collected, at 5, 10.5, 16.5, 23, 31.5, and 42.5 hours, respectively. The nutrient and exudate samples were analyzed by three methods: (i) Iron was determined after ashing at 500°C by *o*-phenanthroline (*7*) and reported as total iron. (ii) Iron as FeEDDHA was determined spectrophotometrically at 480 mμ. (iii) The chelating capacity of nutrient and exudate was determined by the addition of 2.9 μg of $Fe^{55,59}$ (specific activity, 937 count/sec μg) to each 1-ml sample, followed by adjustment to *p*H 6, heating to 90°C, and equilibration for 12 hours. Samples were then adjusted to *p*H 8, heated, filtered (No. 42 Whatman) into planchets, slowly dried, and counted in a proportional counter. The amounts of radioiron chelated were taken as an index of the chelating capacity of the samples.

The absorption of iron by sunflower roots is reflected in the decrease in iron in the nutrient and the increase in iron in the exudate (Table 1). With the loss of iron from the nutrient, the chelating capacity of the nutrient increased sevenfold. This suggested that iron-free EDDHA was remaining in the nutrient. To test this possibility, excess iron was added to samples of pre- and postnutrient. The resulting increase in absorbance values (Fig. 1) shows that the iron added to the postnutrient was chelated by EDDHA. Addition of iron to the prenutrient caused very little change.

From these experiments it is concluded that iron and EDDHA are not absorbed by sunflower plants in equivalent quantities but that iron is released to the roots and most of the EDDHA remains in the nutrient solution (*8*).


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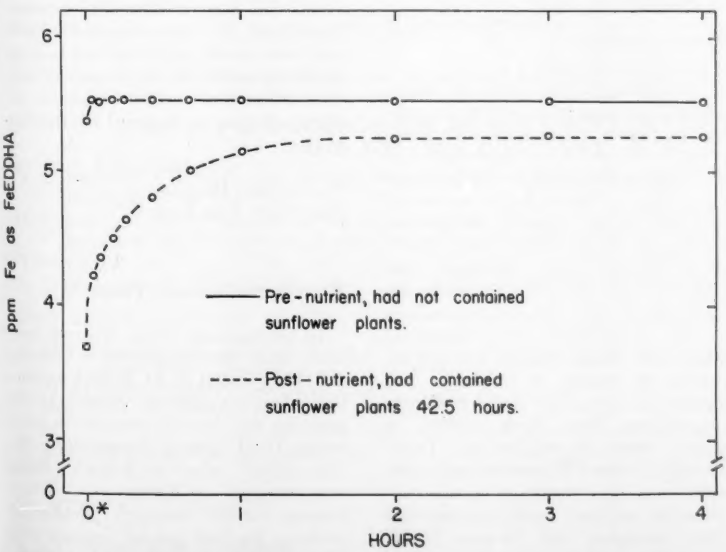


Fig. 1. Rate and extent of chelation of iron as FeEDDHA at 480 mμ by a nutrient solution before and after it contained sunflower plants. Excess $Fe^{+++}$ was added to pre- and postnutrient samples at zero time (*).

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8. Experiments with other plant species lead to the same conclusion. This work is being done in cooperation with the University of Maryland.



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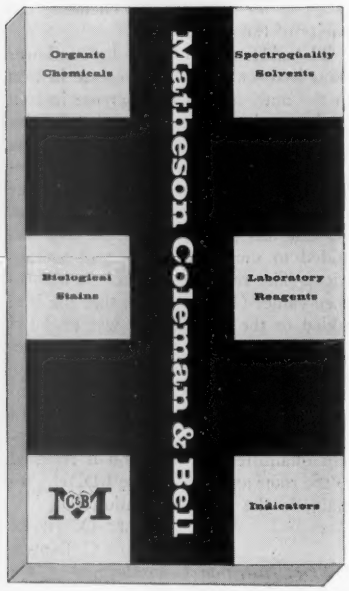


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# Letters

## Ashkenazic Jews

The article by P. H. Saldanha and W. Beçak, "Taste thresholds for phenylthiourea among Ashkenazic Jews" [*Science* **129**, 150 (1959)] is a good presentation of data relating to thresholds and correlated information. The data obtained appear to be highly selective insofar as the groups discussed are concerned. The comparison is poor, since the Danish and English groups representing Europeans (Caucasian race) are in no way related. The Jews were not permitted in the Scandinavian countries until 1904; they were in England for approximately 200 years, were expelled in the 12th century, and were later readmitted in the 18th century. This may appear to justify the comparisons.

Historical facts exist to show that the Polish Jews did intermarry with their neighbors and did change their faith to intermarry later. Strong evidence in support of these facts may be found by examining heraldic symbols of the Polish nobility. A closer relation can be found between the German Ashkenazic Jews and the German people, since intermarriage occurred more frequently.

The specific problem of racial difference or similarity, which occupied the German racial theorists from 1933 to 1945, may be indicated by a comparison of the neighbors of the Ashkenazic Jews, as well as by comparisons with the Chinese and Japanese (Mongoloid race). This still leaves unanswered the question of the relation of the Mediterranean peoples (Spanish, Italian, and Greek) to the Jews.

HOWARD G. LASSER
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The comments by Lasser are in accordance with my own views of the problem of Jewish relationships with Europeans as far as I have understood Lasser's criticisms. I think Jewish intermarriage with European people has varied to a great extent according to the country considered. Available historical data could indicate that Ashkenazic Jews from Central Europe (including the Polish ones) have received appreciable genetic contributions from Europeans other than Mediterranean, but the invasions of Europe by Mongols [see Carleton S. Coon, *The Races of Europe* (Macmillan, New York, 1939)] at several times (especially the Tartar invasion in the 7th century, when the upper class of a khazar group was converted to Judaism) could have injected some admixture into Western Jewish communities (mainly into those from Poland and Russia). Since there are no available data on taste ability to PTC obtained by the same technique among non-Jews from Poland, it appears reasonable to me to compare Polish Jews with English and Danish peoples who could represent typical non-Mediterranean Europeans, in order to know how great the Jews depart from the latter. Moreover, as is pointed out in the article, the relationship of Ashkenazic Jews to Negro groups on the grounds of Rh blood type (see Mourant; see references in the article) must be considered too. Efforts will be made to collect data on other Jewish groups elsewhere.

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## The Green Ray

In his review of O'Connell's *The Green Flash and Other Low Sun Phenomena* [*Science* **129**, 1218 (1959)], Charles H. Smiley bemoans the fact that hardly anybody knows of the existence of the green ray. This is not true. Any red-blooded youngster of a generation ago has read Jules Verne's *The Green Ray* (which is listed in O'Connell's bibliography). Scottish belief has it that only one who has seen this phenomenon knows whom he or she truly loves, and Verne's heroine, a capricious lady of Edinburgh, refuses to marry until she has beheld it. All concerned set out on an expedition to the island of Staffa, famous for Fingal's cave. Eventually everyone sees the green ray except the lady and her lover, who are busy looking into each other's eyes.

My own search for the green ray, which has led me, too, to the island of Staffa, has been fruitless (nevertheless, I married). The green ray can be seen only when one views the horizon from an elevation, and this—plus factors of latitude, season, and weather—probably explains why few sea captains are familiar with it.

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## Response in Nervous Tissue

In his excellent article "Neuron doctrine and electrophysiology" [*Science* **129**, 997 (1959)], T. H. Bullock surprisingly does not refer to Bishop's earlier summary and conjectures on nerve physiology [G. H. Bishop, *Physiol. Revs.* **36**, 376 (1956)]. Much of Bullock's thesis was proposed by Bishop as a unitary concept for the seemingly paradoxical evidence for both graded response and all-or-none response in nervous tissue. Bishop's major points were: (i) that the all-or-none response is a special case of the general property of excitability; (ii)

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that all-or-none conduction was developed chiefly under the necessity of action over distances in organisms whose parts became separated by reason of size and complexity and where graded response and consequent decremental conduction would be inadequate for central-nervous-system control of peripheral organs; and (iii) that the chief characteristic function of nervous and other excitable tissues are performed by means of graded responses.

LAWRENCE R. PINNEO
*Allan Memorial Institute of Psychiatry, McGill University, Montreal, Canada*

# Meetings

## Southwestern and Rocky Mountain Division

The Southwestern and Rocky Mountain Division of the American Association for the Advancement of Science held its 35th annual meetings in Laramie, Wyo., 5–9 May 1959. Most of the sessions were conducted jointly with those of the 30th annual meetings of the Colorado-Wyoming Academy of Science.

Featured sessions at the meetings included an address by Chauncey D. Leake, president-elect of the AAAS, on "Standards of measurement and nursery rhymes"; a report on the activities of the association by Paul D. Klopsteg, president; and an invited paper, "Physical constants as standards of measurement," by A. G. McNish of the National Bureau of Standards.

Programs of the sections of the division and of the Colorado-Wyoming Academy of Science included 137 individual papers. Two symposia consisting of invited papers were conducted. One of these, sponsored by the division's committee on desert and arid zones research, was presented by eight specialists in the fields of agriculture, on the subject "Agricultural problems in arid and semi-arid environments." At the fourth in a series of symposia on improvement of science teaching, some problems of college teaching were discussed. After a stimulating talk on "Graduate training for the college science teacher," by Robert H. Bruce, dean of the Graduate School of the University of Wyoming, discussion groups, divided according to scientific discipline, considered their special problems.

On the final day of the meetings a large group of members of the Colorado-Wyoming Junior Academy of Science attended sessions featuring a chalk talk on the evolution of the Rocky Mountain area, by Samuel H. Knight of the University of Wyoming, and presentation of student papers.

The division's annual John Wesley Powell memorial lecture was presented by Hershel K. Mitchell, professor of biology of the California Institute of Technology, who spoke on "A perspective of biochemical genetics."

Retiring divisional president Albert R. Mead, professor of zoology of the University of Arizona, delivered the presidential address, on "Science and extremism."

The newly elected officers of the division include Lora M. Shields (New Mexico Highlands University), president; Alan T. Wager (Arizona State University), president-elect; and Earl D. Camp (Texas Technological College), member of the executive committee. Marlowe G. Anderson (New Mexico State University) will continue as executive secretary-treasurer and council representative.

Sul Ross State College at Alpine, Texas, was chosen as the site of the 1960 meetings, and an invitation from Arizona State University to hold the 1961 meeting in Tempe, Arizona, was accepted.

MARLOWE G. ANDERSON
*New Mexico State University, University Park*

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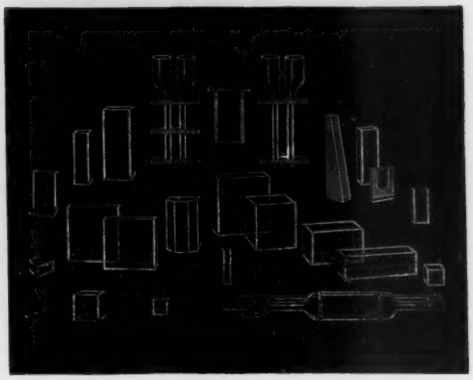

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To be published in July 1959:

Volume 14 — Supplement V

# STEROIDS III
# OXO-COMPOUNDS

Under the auspices of Beilstein-Institut für Literatur der Organischen Chemie. Edited by F. RADT
Pages 2215—2990 Gr.-8°. 1959. Cloth DM 398.—

*When, a few years ago, the publication of Elsevier's Encyclopaedia of Organic Chemistry was suspended, it seemed desirable to complete—on account of its monographic character—the steroid section in accordance with the original conception. One consideration, among others, in favor of this was the fact that the steroid section of Beilstein's Handbuch will not be available for some years to come. Thus, the present publication will be a valuable help in bridging the gap for those already using Beilstein. In view of the undiminished activity in this special field—equally important to chemists, biochemists, and the medical world—the hope that the completion of this section will meet the needs of a wide circle of readers appears to be justified.*

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## Forthcoming Events

### August

*30–3.* American Inst. of Biological Sciences, annual, University Park, Pa. (H. T. Cox, AIBS, 2000 P St., NW, Washington 6.)

*30–4.* American Cong. of Physical Medicine and Rehabilitation, Minneapolis, Minn. (Miss D. C. Augustin, 30 N. Michigan Ave., Chicago 2, Ill.)

*30–4.* Laurentian Hormone Conf., Mont Tremblant, Quebec, Canada. (G. Pincus, 222 Maple Ave., Shrewsbury, Mass.)

*30–4.* Medical Education, 2nd world conf., Chicago, Ill. (World Medical Assoc., 10 Columbus Circle, New York 19.)

*30–5.* World Federation for Mental Health, 12th annual, Barcelona, Spain. (Miss E. M. Thornton, Secretary-General, WFMH, 19, Manchester St., London W.1, England.)

*30–6.* History of Science, 9th intern. cong., Barcelona and Madrid, Spain. (J. Vernet, via Layetona 141, Barcelona.)

*30–6.* Residues on Crops and/or the Problem of Insect Resistance to Insecticides, symp., Munich, Germany. (R. Morf, Secretary-General, IUPAC, c/o Sandoz S. A., Basel, Switzerland.)

*30–6.* Thermodynamics and Experimental Thermochemistry, 17th intern. cong. (Intern. Union of Pure and Applied Chemistry), Munich, Germany. (Div. of Chemistry and Chemical Technology, Natl. Research Council, Washington 25.)

*30–12.* International Oceanographic Cong. (AAAS, UNESCO, ICSU), New York, N.Y. (Miss M. Sears, chairman, Woods Hole Oceanographic Institution, Woods Hole, Mass.)

*31–2.* Free Radical Stabilization, 4th intern. symp., Washington, D.C. (A. M. Bass, Natl. Bureau of Standards, Washington 25.)

*31–2.* Stratospheric Meteorology, conf., Minneapolis, Minn. (H. G. Houghton, AMS, Dept. of Meteorology, Massachusetts Inst. of Technology, Cambridge 39.)

*31–3.* Biological Photographic Assoc., Montreal, Canada. (Miss J. H. Waters, P.O. Box 1668, Grand Central Station, New York 17.)

*31–3.* Mathematical Assoc. of America, 40th summer meeting, Salt Lake City, Utah. (H. M. Gehman, MAA, Univ. of Buffalo, Buffalo 14, N.Y.)

*31–4.* Haematin Enzymes, symp. (by invitation), Canberra, Australia. (A. H. Ennar, John Curtin School of Medical Research, Australian National Univ., Canberra.)

### September

*1–3.* Association for Computing Machinery, natl., Cambridge, Mass. (J. Moshman, Council for Economic and Industry Research, Inc., 1200 Jefferson Davis Highway, Arlington 2, Va.)

*1–6.* College of American Pathologists, Chicago, Ill. (A. H. Dearing, Suite 2115 Prudential Plaza, Chicago 1.)

*1–7.* History and Philosophy of Science (General Assembly, History Div., Intern. Union of the History and Philosophy of Science), Barcelona, Spain. (R. Taton, IUHPS, 64, rue Gay-Lussac, Paris 5e.)

*1–8.* Acoustics, 3rd intern. cong., Stuttgart, Germany. (E. Zwicker, Breitscheidstrasse 3, Stuttgart N.)

*1–7 Oct.* International Civil Aviation Organization (Meteorological Div.), Montreal, Canada. (ICAO, Maison de l'Aviation Internationale, Montreal.)

*2–4.* Allergy, 4th European cong., London, England. (British Assoc. of Allergists, Wright-Fleming Inst., St. Mary's Hospital, London, W.2.)

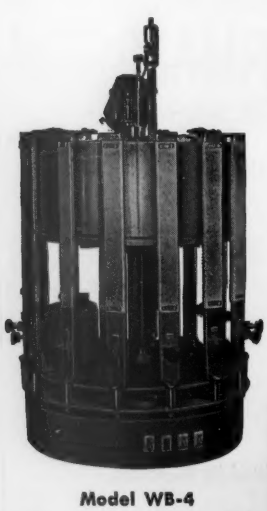
*2–4.* Cryogenic Engineering Conf., Berkeley, Calif. (K. D. Timmerhaus, CEC, Chemical Engineering Dept., Univ. of Colorado, Boulder.)

*2–4.* Crystal Imperfections and the Chemical Reactivity of Solids (Faraday discussion), Kingston, Ontario, Canada. (Faraday Soc., 6 Gray's Inn Sq., London, W.C.1, England.)

*2–5.* American Mathematical Soc. and Mathematical Assoc. of America (joint summer), Salt Lake City, Utah. (E. Pitcher, AMS, Lehigh Univ., Bethlehem, Pa.)

*2–8.* Foundations of Mathematics: Infinitistic Methods, symp., Warsaw, Poland. (A. Mostowski, Dept. of Mathematics, Univ. of California, Berkeley 4.)

*(See issue of 19 June for comprehensive list)*

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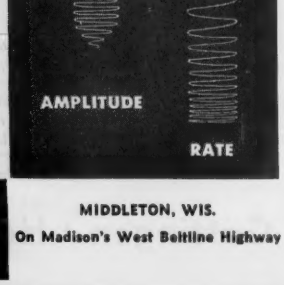


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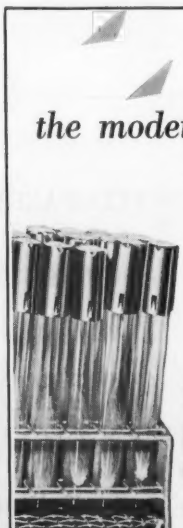

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■ VOLATILES TESTER provides moisture or volatile-content readings with accuracy said to be ± 0.05 percent. Operation is semiautomatic. Ten-gram samples are weighed in special pans and, after any given drying period, moisture percentage may be read directly. (C. W. Brabender Instruments Inc., Dept. 942)

■ TEAR TESTER is designed to measure tearing strength of strongly directional materials that tend to tear at an angle. The tester holds the specimen rigidly throughout its entire length on both sides and directs the tearing force to produce a tear down the center line of the specimen. (Thwing-Albert Instrument Co., Dept. 943)

■ VOLTAGE DIVIDER for alternating current is claimed to provide linearity better than ± 0.0001 percent over a wide range of audio frequencies. Seven decades of division are provided by specially constructed toroidal transformers. (Electro Measurement Inc., Dept. 945)

■ TAPE RECORDER for airborne application weighs 24 oz. Tape widths are ½ and 1 in., and seven tape speeds from 0.25 to 15 in./sec are provided. Wow and flutter are less than 1 percent under static conditions. Environmental operating ranges are: acceleration, 200 *g*; temperature, −50° to +200°F; relative humidity, 0 to 100 percent. (Leach Corp., Dept. 946)

■ INDUCTANCE BRIDGE measures inductance from 0.002 μh to 100 mh. Resolution of approximately ± 0.01 percent and accuracy of the order of ± 0.25 percent are claimed. For excitation, an internal oscillator provides signals of several frequencies between 1 and 100 kcy/sec. Detector and null indicator are also self-contained. Series resistance can be measured in the range 0.01 to 10,000 ohm. External oscillators and detectors may be used. (Boonton Electronics Corp., Dept. 947)

■ RESISTANCE-RATIO RECORDERS are designed to measure pressure, flow, and other variables detected by three-terminal, potentiometer-type transducers having total resistance between 1000 and 10,000 ohm. The ratio circuit uses the transducer as one-half of a Wheatstone bridge circuit and the measuring slidewire of the recorder as the other half. Standardization is not necessary because


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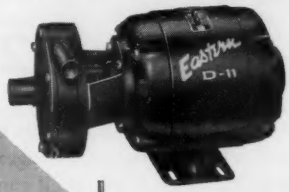


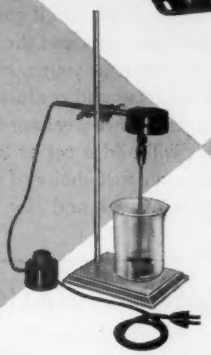

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## Chicago, 26-31 December 1959

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If desired, the hotels will add a cot at $3.00 per night—except that all children under 12 are free. Mail your application *now* to secure your first choice of desired accommodations. All requests for reservations must give a definite date and estimated hour of arrival, and also probable date of departure.

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For a list of the headquarters of each participating society and section, see page 228, *Science*, 24 July.

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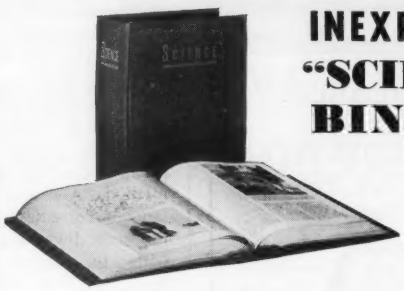

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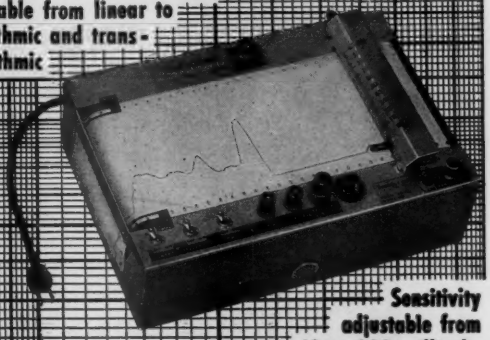

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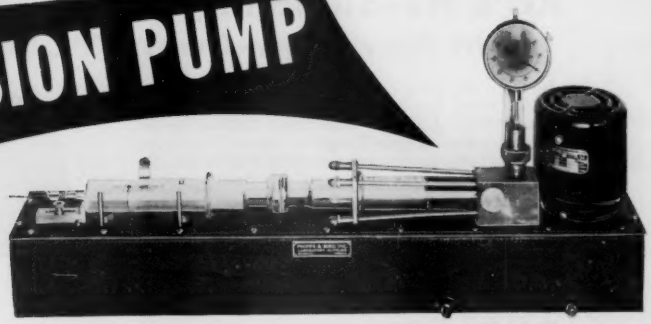

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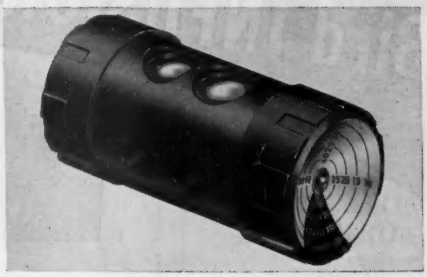

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